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THERMODYNAMICS AND THE SUBJECTIVE SENSE OF TIME

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1 '*Time's Arrow*'

EDDINGTON [1] regarded the second law as providing a criterion of the direction of time which is independent of any subjective element. He wrote: 'Without any mystic appeal to consciousness it is possible to find a direction of time on the four-dimensional map by a study of organisation. Let us draw an arrow arbitrarily. If as we follow the arrow we find more and more of the random element in the state of the world, then the arrow is pointing towards the future; if the random element decreases the arrow points towards the past.'

This well-known passage is not as clear as it might seem; it is not apparent whether Eddington is saying (*a*) that it is a fact of observation that the later instant is always found to correspond to a greater entropy,[2] or (*b*) that the second law may be used as a *definition* of the direction of time, that is 'later' is the same as 'more random'. The first of these meanings would imply that we have some criterion which is independent of the second law, concerning which of two instants is the later; however, it is well known that no other law of science will provide this and, if the subjective sense of time is to be excluded, we are left with no other guide by which to make this decision. Thus only the second meaning is self-consistent. But if so, is there any physical content in the law, or is it a tautology?

The actual situation, I think, is that each scientist must first come to a personal conviction, on the basis of his own experience, of the truth of the second law ; for this purpose he depends on his subjective judgement concerning which of two events precedes the other. It is only when he is convinced that the law may be applied consistently to a great many different kinds of spontaneous change, that it becomes something more than tautological to use it as an objective definition of ' time's arrow '. These points may be expressed in more detail as follows.

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[1] Eddington, *The Nature of the Physical World*, Cambridge, 1927

[2] In the quoted passage, the word 'random' was used by Eddington for the purpose of a pictorial representation of entropy.

The second law (in its original and non-statistical form) is based on the empirical fact that certain changes of state are impossible in an isolated system. In this statement the notion of time's arrow has already been implied ; thus if I say that the change $B \rightarrow A$ is impossible, I mean that the state A (of an isolated system) can never be later than state B, although A may be earlier than B. Assuming the meaning of ' earlier ' and ' later ' to be understood, and in the absence of any other criterion, it is myself who judges, in fact, that the A state never *is* later than B. For example, in Joule's well-known experiment for the determination of the mechanical equivalent of heat, a weight falls slowly to the ground and, through the action of a string and pulley, it causes a paddle wheel to revolve inside a quantity of water, which thereby rises in temperature. Whatever is done to the system, provided that it remains adiabatically enclosed, the observer is never conscious of seeing the reverse of this process ; he never sees the water getting cooler and the weight rising, relative to his personal decision concerning the time sequence. Thus let A be a state of the system corresponding to a height, h_A, of the weight and a temperature, T_A, of the water and let B be another state corresponding to h_B and T_B. If $T_B > T_A$ it seems that the human observer always decides that the B state is later than the A state [1]—i.e. $t_B > t_A$, where t is the time. The impossibility of restoring the system to its original state, without transferring heat to some other body, is an entirely sufficient basis for the second law—that is the existence of entropy as a function of state, and its increasing property. This increase, however, is relative to the subjective time sense, taken as the (apparently) independent criterion concerning which is the later instant ; at any rate this is the case whenever the law is derived *ab initio*.

It may be suggested, perhaps, that the subjective element might be avoided by the use of clocks and calendars. I will suppose that I am on a desert island, trying to keep a record of what happens by the use of a primitive calendar. Let A and B be two events, for example the flowering and decay of a plant, and suppose my consciousness tells me that B is later than A. In order to establish this result by a less subjective method I construct for myself a calendar by making a notch on a stone at every recurrence of a periodic process, such as the sun crossing the horizon. It might appear, therefore, if I noted event A as taking place when there were, say, ten notches on the stone and event B as taking place when there were forty notches, that I had found an *objective* criterion that B was later than A. Consider, however, the process of making the nth notch on the stone. Let the state in which there are $n—1$ notches be called α and the state in which there are n notches be called β. In the construction of the calendar I have already assumed, on the basis of my subjective time sense, that β is later than α; that is, that the number of notches is increasing and not decreasing or, expressed in another way, that the action I carry out is one of notching and not of denotching.

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[1] The additional fact that if $T_B > T_A$ then $h_B < h_A$, is concerned with the first law and not with the second.

Thus I can only decide *objectively* that B follows A if I have already decided *subjectively* that state β of the calendar follows state α. To make the latter decision objectively would require a second calendar, and would thus lead to an infinite regress. The same considerations apply to clocks. As regards time's direction, as distinct from its measurement, it must be decided by the human observer that the clock hands are moving in the one direction and not in the reverse, or that the sand is falling in the hour-glass and not rising. Similar difficulties would apply, I think, to any suggestion that the later of two events might be defined objectively by use of the notion of causal chains, or on the sending of signals from one point to another. For example, the sending of a light signal requires an absence of thermodynamic equilibrium and the temporal sequence is already implicit when it is supposed that the lamp is a device for emitting radiation rather than absorbing it.

It seems that the only way in which the subjective time decision may be eliminated is by using the second law itself. Nature provides us with a very large number of dissipative changes : processes of heat transfer and mixing, and the thousands of chemical and nuclear reactions. As soon as the scientist has convinced himself that in each of these the entropy increases, when it occurs in an adiabatic enclosure, it becomes possible to choose any one of them as a reference. Thus we might choose as our standard a sample of radium and, of two instants t_i and t_j, the later would be defined as the one at which the quantity of radium was less. This provides an objective criterion of 'time's arrow' and it seems that it is in this sense that Eddington's remarks are to be understood. This use of the second law for the purpose of a definition does not imply that the law loses its physical content; this is because of its consistency as applied to *all* kinds of process. A distinction between past and future as defined for any one type of macroscopic adiabatic change, has physical significance for all other types.[1]

2 *Reversal of Time's Arrow*

As is well known, statistical mechanics shows that the second law is a probability rather than a certainty; thus of several processes, in adiabatic systems of finite size, it is highly probable, but not quite certain, that they will all show a parallel change of entropy.[2] There is always a slight chance that one of them may 'misbehave' and undergo a reversal relative to the others. Thus we cannot claim that any single reference system is an absolutely reliable guide to 'time's arrow'; but the same objection could be raised against the subjective criterion.

In the context of the present article a more interesting question arising from the statistical theory is concerned with the possible repetition of identical states of the universe itself.[3] Any isolated body, according to the theory, takes up all of its stationary quantum states again and again; the same may be said of the universe, if it is a closed dynamic system. Thus if it is in a state A at time t, the same state $A'(A' = A)$ may be expected to occur at some other time t'. This view has been widely held since Boltzmann and Smoluchowski, and a typical statement is that of G. N. Lewis:[4] 'The precise present state of the universe has occurred in the past and will recur in the future, and in each case within a finite time.'

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[1] This point of view with regard to time's *direction* is closely analogous to that which is normally adopted with regard to its *measurement*. Equal intervals of time are defined in terms of equal intervals of space, together with the consistency of the laws of motion. Perhaps a more logical procedure would be to use the second law as the basis for both the direction and the measurement of time. In fact an appropriate time scale has already been developed by Dingle (*Phil. Mag.*, 1944, **35**, 499), using a relativity theory of the thermodynamics of radiation.

[2] In this connection a recent paper by Schrödinger (*Proc. Roy. Irish Acad.*, 1950, **53a**, 189) is of great interest.

[3] Or that part of it which is observable.

[4] G. N. Lewis, *Science*, 1930, **71**, 569

In regard to an identical repetition, the difficulty which seems not to have been faced is this : How is the repetition to be observed, or how is t' to be distinguished from t? By hypothesis A' is the same as A. Everything about the universe is the same in the two states, including all written records of events. There is thus no objective criterion that t' is any different from t—that is, that any time has elapsed between A and A'. If the repetition is complete, the same identity of states must apply to the human observers, *including the content of their memories.* It seems, therefore, that the subjective criterion is also excluded. There is thus no sense in the statement that t' is a different instant from t.

In short, our belief that time has elapsed is always based on the observation of states which differ in at least *some* repect from all other experienced states, even if it is only in the accumulated memory of those previous states. In laboratory thermodynamics we can make meaningful statements about the reversibility of a process only because the observer does not consider himself as being reversed simultaneously. We stand outside the laboratory apparatus, equipped with our undirectional sense of time. But we do not stand outside the universe ; states which appear to us to be identical in all respects cannot be observed twice, but are in fact only a single state at one instant. Thus a continuous line of human beings can follow the development of their environment, and of themselves, in one direction, but not also in the reverse direction. We ourselves, in this year of grace, have no possible knowledge whether we are repeating a previous identical state. The reversibility of the universe is thus a logical deduction from a particular model, but at the same time it is unobservable [1] in principle and is philosophically meaningless.

3 *The Subjective Time Sense*

How are we to explain the linkage between the subjective time sense and the metrical t-co-ordinate of physics ? To the scientist it seems more natural to explain the former in terms of the latter, rather than the converse. Thereby we discuss the time sense of the human being in terms of a system of already well-established laws concerning the duration and irreversibility of external events. The converse would lead us into no-man's land.

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[1] From this point of view the hypothesis of a continuous and irreversible creation of matter serves to eliminate the unobservable.

However, it must be admitted that the subjective and objective aspects of time are intermingled to a high degree. The notion of change, on which our concept of time is based, is not one which can be examined externally ; we ourselves are a part of the changing world ; we do not stand outside the temporal process which we seek to discuss. The situation is thus closely analogous to one in psychology, where it is impossible for a man to examine his own mental states as if from outside.

It seems best, therefore, to avoid any sharp distinction between subjective and objective time, but instead to regard the human being, in his physical aspects, as taking part in the same processes which occur in the external world. Indeed we feel that we do not merely *observe* events, as if they existed in advance along a time dimension ; we believe that we can *cause* them to take place, through volition and action. We do not observe the world, we take part in it.

Now the decision which of two events precedes the other depends on the faculty of memory,[1] which enables us to compare what *is* with what *was*. However this is not the whole story, for how do we distinguish the *is* state from the *was* state ? What is the peculiarity of the present state by which we know that it is the present, and not merely some remembered state which may be almost equally vivid in the mind's eye ? The present has, I think, as its distinctive feature a sense of being physical and tangible, of being correlated to the vital activity of the organism. It is the living now, in which I meet other creatures, as it were, at a common point, the state of participation in a single process. The remembered state, on the other hand, has an inactive and private character ; it is not associated with any feeling of purpose or the necessity of taking action.

The key to the situation, I think, is the idea that we participate, in our own bodies, in the same chemical and physical processes which occur in the external world. Regarding these processes as the reality, the time sense of a human being derives from their existence within him, giving him the feeling of an inner time not based on the five senses.

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[1] Russell remarks ' there are two sources of our belief in time ; the first is the perception of change within one specious present, the other is memory '.

From this point of view there are two things to be explained :

(*a*) the consistency of time's direction between the subjective sense on the one hand, and the external events on the other ;

(*b*) the corresponding consistency in regard to the metrical aspect of time, that is, the approximate agreement between our own estimates of short durations and those based on the clock.

The first of these is equivalent to the question, Why are the subjective and objective criteria of the sequence of events really in accord, as discussed in Section 1 ? The answer may reasonably be based on the fact that the development of human beings is unidirectional, like the rest of nature. Whether the entropy of the human body increases or decreases is beside the point ; it may do either in complete conformity with the second law because the body is an open system and causes large entropy increases in its environment. The important thing is the irreversibility of our own development from birth to death. It is a commonplace that a year seems much shorter to an old person than to a child. To a child of ten, a year is a tenth part of its total development, but to a man of seventy it is only a seventieth part.[1] But in each case it is a year in the same direction ; a year of continued aging in the body and of a continued increment of memory in the mind.

With regard to the sense of duration—the second of the problems raised above—the accuracy is often remarkable, especially during hypnosis. Myers[2] has written : 'The records of spontaneous somnambulism are full of predictions made by the subject as to his own case, and accomplished, presumably by self-suggestion, but without help from clocks, at the precise minute foretold.' But even without hypnosis we can usually set ourselves to waken at some unusually early hour, for example, to catch a train, without the use of an alarm clock. Also, on wakening at night it is usually possible to guess the time, to within about half-an-hour, before looking at a watch.[1] The same applies during ordinary day-time activity, but is disturbed by pleasure or boredom.

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[1] This inverse relationship has suggested the utility of a biological time τ which is related linearly to the logarithm of ordinary clock time t. Reference may here be made to the well-known work of Lecomte du Nuöy (*Biological Time*, London, 1936), on the rate of healing of wounds, which varies approximately inversely as the age of the patient ; to some later researches of Backman (*Roux's Archiv. für Entwicklungsmechanik*, 1940, **140**, 90) concerning the ages at which various animals pass through distinct phases of their development ; and finally to some recent work of Prigogine (*Étude thermodynamique des phénomènes irréversibles*, Paris, 1947), who has shown that chemical reactions, when they are close to equilibrium, give approximately equal increments of entropy in equal increments of τ.

[2] Myers, *Human Personality*, edition of 1907, page 69. This is also discussed by Gunn, *The Problem of Time*, Ch. VII, London, 1929.

In explanation of this faculty it may be suggested that the succession of night and day has a strong regulating effect on the physiological system of the body, as shown, for example, by the need for sleep with a frequency equal to that of the sun. The organism is also an open system which maintains itself at the expense of its foodstuffs. Some parts of the metabolism are often in an approximately steady state, and the rate of entropy creation, due to the chemical reactions, is approximately constant relative to the sun.

Some evidence in favour of this idea of chemical time-keeping by the body has already been obtained. François,[2] for example, measured against a watch the rate at which persons with normal and abnormal temperatures tapped on an electric switch at a rate which they believed was three times per second. The higher the body temperature the greater was the rate of tapping, indicating a more rapid sense of time's passage, relative to 'clock time'. The results were extended and confirmed by Hoagland.[3] Over the relatively small range of temperatures which could be explored, it seemed that the temperature coefficient of counting was much the same as is usually observed in chemical reactions—that is, a two- or three-fold increase per 10° C.

Somewhat similar results have been described by Munro Fox.[4] It is known that bees can be trained to visit a dish of honey at a set time each day, but the accuracy of their time keeping can be disturbed by changing their physiological activity. For example, if dosed with thyroid extract they come too early and if dosed with quinine they come too late.

There is thus a certain amount of evidence that the sense of time in the organism may have its origin in chemical reactions; their irreversibility is in accordance with the second law and their speed is regulated to that of the sun—i.e. closely to the metrical *t*-co-ordinate of physics. It seems, therefore, that the linkage of subjective and objective time may perhaps be understood through the participation of man's bodily processes in the irreversibility of nature.

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[1] This faculty seems to be lost during illness. Thus a patient will often waken at midnight and ask if it is time for the morning tea.

[2] François, *Comptes Rendus*, Societé de Biologie, 1928, **98**, 201

[3] Hoagland, *Phil. of Science*, 1934, **1**, 351

[4] Munro Fox, *The Personality of Animals*, London, 1940; *Selene, or Sex and the Moon*, London, 1928

Before concluding it is perhaps appropriate to draw a sharp distinction between bodily and mental processes. In a certain sense man's mind has freedom from the temporal sequence, for he is able to project himself backwards in memory and forwards in imagination. The emergence of spirit in living creatures suggests also that there are tendencies in play which are outside the scope of the second law (and are thus not contrary to it), and which indicate the existence of a parallel process of a more hopeful kind. In this movement the significance of time is not the same as it is in science but is related to volition and purpose. Man's spiritual life is an active search; but for the finite being purpose is meaningless without the notion of a future and therefore of time itself.

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ON PHYSICAL MODEL EXPLANATIONS IN PSYCHOLOGY *

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Much current theoretical work in psychology takes the form of looking for appropriate physical models with which to describe, by analogy, the findings of experiment and observation. The ' problem of consciousness ' is shelved, or regarded as a logically different sort of question from those the experimentalist can hope to answer with his techniques. The experimentalist asks instead what sort of mechanism could perform the tasks undertaken by the central nervous system. He considers the ordered activity of the organism in performing a skill, and the changes in performances during the learning of skills, and he considers perception and learning and nervous organisation in terms of practical or theoretically possible machines.

Perhaps the best-known examples of this way of thinking in psychology are the computer and the servo-mechanism analogies. The former was suggested by the feats of the large electronic calculating machines capable of producing numerical solutions to problems requiring perhaps years of work by a mathematician unaided by machines. The latter analogy was suggested during the war, when for example, automatic gun-aiming devices were developed which caused guns to follow moving targets in a manner which seemed similar to eye-hand co-ordination. The servo-mechanism analogy has proved useful both in interpreting the physiological mechanisms responsible for controlled movement, and for the design of machines such as aeroplanes which must be accurately controlled by men. The man is himself regarded as part of the total servo-loop, his parameters are determined (for example the phase angle of the error for various input frequencies),. and these are accepted as part of the data to be considered when designing the control systems of aircraft.

The explicit use of such analogies is limited to the last decade in psychological writing, but the wisdom of regarding the mind as in some sense a machine has been debated at least since the work of

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* Received 14. i. 53

Pavlov. J. B. Watson's[1] attempt to describe behaviour in terms of the conditioned reflex was criticised at the time by writers, particularly those of the Gestalt school, as being too mechanistic.

Psychological theories are traditionally divided into two classes namely: 'atomistic' theories and 'field' theories. The atomists stem from Scottish analytical philosophy, from the attempt to describe experience in terms of complexes of simple elements, while field theories stem from German metaphysics, and its insistence on the doctrine (so important to the Kantian theory of knowledge) of 'internal relations'. It seems important to indicate the origins of these dichotomous approaches if only to suggest the relevance of epistemological considerations to the study of brain function. The experimental psychologist is perhaps apt to ignore the point that his theory of how the brain works must be commensurable with his theory (if any) of how it is *possible* to gain knowledge of the world.

If the relevance of epistemology to the study of brain function be accepted, then some of the difference between the Atomists (or Associationists) and Field theorists (or Gestalt writers) may be discussed by the philosopher as well as by the psychologist.

J. Z. Young makes the point in *Doubt and Certainty in Science*, that it is convenient to talk about brain function in terms of machine analogies because we are familiar with machines. Our language is developed to talk about machines and the properties and behaviour of common objects; we find great difficulty in talking about anything else. This suggests that the psychologists who have not explicitly used machine analogies may well have done so implicitly. I suggest that this is true of the Gestalt school.

Consider the emphasis placed by the Gestalt writers on the 'Principle of Good Configuration' or 'Good Gestalten'. The point which is most stressed by the Gestalt school is the importance of direction in thinking, and in activity and in the processes leading to perception. This directive-tendency is described in terms of the tendency of the physical Gestalten (situated in the brain) to assume states of maximum stability.

Köhler, in his essay 'Physical Gestalten' (1920) discusses the physicist's equilibrium, and dynamic states of physical systems; and he holds that the directive-tendencies of thought and perception are to be regarded as manifestations of the physicist's laws of the tendency of physical systems to adopt equilibrium states. At the same time Köhler objects to talking about 'mechanism' when discussing brain function; he does not want to reduce his physical Gestalten to simpler elements.

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[1] *Psychology from the Standpoint of a Behaviorist*, London, 1919; *Behaviorism*, London, 1925

The Gestalt school's objection to 'mechanical' models is, perhaps, responsible for masking the surely indisputable fact that the postulated physical Gestalten—with the physical laws supposed to describe their properties—constitute a machine analogy of brain function. It seems that the Field theorists are not averse to mechanical analogies, but they choose larger units than do the Associationists. The often-quoted phrase: 'The whole is more than the sum of the parts', with which Gestalt writers sum up their objection to the Associationist's analysis of, for example, the perceptual field, clearly indicates their point of view. The status of their objection is another matter and depends upon the philosophical analysis of the phrase, the whole is greater than the sum of the parts; and in particular upon whether it be regarded as a fact about the world that essentially new properties emerge upon the combination of elements or whether, on the other hand, this 'newness' is not rather surprising but in principle predictable. In other words, is emergence a fact about the world (and thus of ontological status), or is it epistemological—the result of our ignorance about how to analyse wholes into parts so that the whole is completely described? I do not propose to dwell or to make any decision on this point, but it is important to make the problem explicit if we are to avoid confusion in our attempt to compare Field with Atomistic theories.[1]

We have said, then, that both the Atomists and the Field theorists adopt physical analogues as explanatory concepts for psychology. Perhaps the recognition that they are both machine analogies makes comparison more fruitful. An essential difference between them is the size of the units chosen, and thus the function of the unit elements in the brain mechanism, which both regard as obeying physical laws which in some sense underlie psychology. The Gestalt writers might prefer to think of some psychological phenomena 'emerging' from the physical states, this emergence being in principle unexplainable in simpler terms, but they certainly use the physicist's laws describing equilibrium states as explanatory concepts for their Field theory.

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[1] For a discussion on Emergent Properties see C. D. Broad, *Mind and its Place in Nature*, London, 1925

The atomists also use physical laws for explanations of psychological findings. As we have said their units are smaller ; they talk of ' switches ' and ' connections ', for example, in their postulated brain mechanisms. Their procedure is to consider which machines (actually or theoretically possible) behave in a manner similar to some isolated feature of the organism's behaviour, such as, for example, learning or perception. They then suggest that there are in the brain physiological correlates of the switches, relays or whatever comprises the machine.

In the case of thinking, the Atomists compare the organism with computers, particularly to large electronic machines such as the E.D.S.A.C. The processes taking place in the organism are supposed to be similar to those taking place in the machine ; the neurones, or groups of them, are supposed to function like the basic ' flip-flop ' circuits. As in the Gestalt physical analogy we have stable states, but here these correspond to the store of each bit of information which is being operated on, perhaps according to logical rules, by the interrelated activity of the switches.

Now it is well known that computing machines are classified into two groups : analogue machines, and digital machines. The former work by providing a continuously variable physical analogue of the function under consideration ; the most common example is the slide-rule. A further example is the mechanical integrator. Digital machines work in small steps each representing a unit logical step. Physically, these steps may be represented by the teeth on gear wheels, as in the desk computer, or by the stable states of such circuits as the ' flip-flop '.

It is the contention of this paper that the Field theories of brain function bear the same relation to the Atomist theories as the analogue computing machines bear to the digital machines. If this be accepted, it seems possible to discuss the rival types of theory in terms of the advantages and disadvantages of the two types of machines as they are used for various purposes. To pursue this theme in detail would require more technical knowledge of these machines than the writer possesses, but we may suggest the chief advantages of the two types.

The digital machine has the following advantages :

1. Flexibility. (It will solve any problem which does not require logical operations which are not built in. Further limitations may be imposed by the finite ' storage ' of any physical machine.)

2. Accuracy. (Provided the machine does not actually make a false step the answer will be correct; analogue machines are subject to random error throughout. Further, their accuracy is dependent on the precision of their construction, this is not true of digital machines.)

The analogue machine has the following advantages:

1. Speed. (Though their range of function is limited they are fast—hence their use for bomb aiming.)
2. Simplicity. (Typically they are comparatively simple, but this may be because they are normally designed for but one class of problem.)
3. In some cases by changing the physical characteristics of the device it may be made to give solutions for various functions.

This list of advantages and disadvantages of the two types of machine may now be compared with the requirements of the brain mechanism. In general we may say that where speed is important the analogue machine is the best bet; but the choice must be influenced by the way in which the mechanism of learning is regarded. The Gestalt school stress the importance of innate tendencies in perception and thinking, while the Atomists stress past experience; the mechanism of learning and storage of information must be regarded as very different in the two theories.

For the Field theorists learning will constitute modifications in the physical characteristics of the analogue device. Information will be stored in a form akin to a collection of cams of various profiles, which may be substituted one for another. Anticipation, or 'set', might be a pre-selection of the appropriate analogue cam.

Learning for the Atomists will be similar to the building up of the libraries of punched tape found in large computing laboratories. These tapes are punched with the steps required for many types of problem; they are used to save time where similar problems have been solved before.

Enough has been said to indicate how the principles of computers may be taken over as languages to describe brain function. To the writer it seems important to call attention to the analogue-digital distinction, and its apparent correspondence in the Field *v.* Atomist controversy. Perhaps this suggests what to look for to make a decision on what is commonly regarded as a fundamental issue in psychological theory.

This way of thinking suggests that there is a *hiatus* in the

Gestalt writers' use of the analogue machine as the paradigm of perception and thinking. The use made of the tendency to equilibrium of the physical Gestalten seems to involve the assumption that the spatial form of the physical Gestalten bears approximately a one-to-one relation with (in the case of perception) the object in the stimulus field. For example, the physical Gestalten related to a perceived circle is supposed to be itself circular in form. If this one-to-one relation is not supposed then the Gestalt thesis that the psychological phenomena of 'closure', etc., can be attributed to the physical properties of the Gestalten is not tenable. There is no obvious reason why the brain should elect to represent a circle with a circular neural modification.

The mathematical techniques of information theory, the notion of the capacity of information channels, and criteria such as economy in the use of neurones, suggest that sensory and other information is likely to be coded in some manner. If this is so then the Gestalt thesis that 'closure' and 'directive-tendencies' can be explained by the physical properties of Gestalten breaks down. If we do not know the code (or the manner in which the transducer works) then we can say nothing whatever about the physical properties of the information store.

An objection to both types of theory, as they are commonly expounded, is that brain function is not explicitly related to the epistemological requirements for gaining knowledge.

An adequate account cannot limit brain function to deductive inference; to gain knowledge inductive generalisation is necessary. Practical calculating machines must be fed with the relevant, and only the relevant, information. This is done by the human operator, but obviously he cannot be included in the analogy. Yet without him the machines will not even begin to produce an answer. We must imagine a machine capable of selecting its own information and devising its own operating rules: it must generalise and develop criteria of relevance. It must be an Inductive machine.

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MODELS IN PHYSICS *

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1. *The Hypothetico-Deductive Method*

THE logical form of the theories of mathematical physics has been described in a previous article ' Operational Definition and Analogy in Physical Theories '[1] where, following N. R. Campbell, F. P. Ramsey and other writers, I have maintained that scientific theories are not constructed solely out of sense-data or out of operational definitions, but are ' hypothetico-deductive '[2] in form ; that is, they consist of hypotheses which may not in themselves have any reference to immediate observations, but from which deductions can be drawn which correspond to the results of experiments when suitably translated into the experimental language. The main point that emerges from such a description of theories is that there can be no set of rules given for the procedure of scientific discovery—a hypothesis is not produced by a deductive machine by feeding experimental observations into it : it is a product of creative imagination, of a mind which absorbs the experimental data until it sees them fall into a pattern, giving the scientific theorist the sense that he is penetrating beneath the flux of phenomena to the real structure of nature. In the present article I shall examine this hypothetico-deductive method further by considering some examples from nineteenth-century mathematical physics—examples which have the advantage for the logician of being comparatively elementary as regards the science and the mathematics, but which demonstrate the typical method of modern science. By means of these examples I hope to bring out two points about scientific hypotheses, the significance of which seems to have been generally overlooked. I shall first state these two points and discuss them in a preliminary way, and then go on to bring evidence in their support from the examples.

I. *Mathematical formalisms, when used as hypotheses in the description of physical phenomena, may function like the mechanical models of an*

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[1] This *Journal*, 1952, **2**, 281

[2] See K. R. Popper, *Logik der Forschung*, Vienna, 1935 and J. O. Wisdom, *Foundations of Inference in Natural Science*, London, 1952, pp. 25 ff.

earlier stage in physics, without having in themselves any mechanical or other physical interpretation. What this means can be explained briefly as follows.

If a hypothesis is to be a satisfactory correlation of a group of experimental data, it must be possible to deduce the data from the hypothesis when the symbols in the latter are suitably interpreted. If the hypothesis is to be a useful instrument of further research, however, this is not the only requirement that it must satisfy. In addition it is necessary that the hypothesis itself should be capable of being thought about, modified and generalised, without necessary reference to the experiments, so that it can be used to predict future experience, or to bring hitherto isolated experimental results into the field of application of the hypothesis. This second condition is fulfilled in the case of the mechanical hypotheses typical of nineteenth-century physics, because they are expressed in terms of mechanical models whose behaviour is known apart from the experimental facts to be explained. The billiard ball model of gas molecules, for instance, consists of a collection of balls moving at random and colliding with each other and with the walls of the vessel, and the behaviour of such a system is already known and expressed in a mathematical theory, independently of the experimental results about gases with which it is compared. This means that further ramifications of the theory of colliding billiard balls can be used to extend the theory of gases, and questions can be asked such as 'Are gas molecules like rigid balls or like elastic ones?', 'What is their approximate diameter?', and so on. Progress is made by devising experiments to answer questions suggested by the model.[1] Similar considerations apply to models from electrical theory. Certain atomic hypotheses, for example, are expressed in terms of a model consisting of charged particles moving under their mutual electrostatic attractions, and the theory of such particles is known independently of the atomic phenomena it is used to explain. Now in a great deal of modern physics on the other hand, we are told that we must not ask for picturable mechanical or electrical models such as these, and that only formal mathematical hypotheses[2] are adequate to express the physical theory. The question then arises, what takes the place in these physical theories of the pointers towards further progress which are provided by an easily pictured mechanical model? I shall suggest that what takes their place is provided by the nature of the mathematical formalism itself—any particular piece of mathematics has its own ways of suggesting modification and generalisation; it is not an isolated collection of equations having no relation to anything else, but is a recognisable part of the whole structure of abstract mathematics, and this is true whether the symbols employed have any concrete physical interpretation or not. Examples of the way this works in the extension of hypotheses will be given in what follows.

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[1] Hutten expresses this by saying that the model provides unformalised 'semantic rules' for the theory. See his article 'On Semantics and Physics', *Proc. Aris. Soc.*, 1948-9, **49**, 115.

[2] Such as the transformation group theory in quantum physics. See the preface to Dirac's *Quantum Theory*, 2nd. edn., Oxford, 1935, p. vi.

It remains to decide the linguistic point, as to whether such abstract mathematical hypotheses are to be called ' models '. It does not seem profitable to try to draw any sharp line between those hypotheses which are expressed as mechanical or electrical or hydrodynamical (wave theories, etc.) models, and those which are purely mathematical. There are many hybrids, like the Bohr model of the atom in which electrons are conceived to jump discontinuously from one orbit to another, a feat which no mechanical particle can be imagined to perform; and again the conception of curvature of three-dimensional space, which cannot be imagined apart from the mathematical formulation, although the imagination can be aided by thinking of a *two* dimensional surface curved in a third dimension. Current physical usage seems to sanction the use of ' model ' for all such cases whether physically imaginable or not. For instance, the various cosmological or world-models[1] are sometimes physically imaginable distributions of matter, but the more recent models involve some element, such as Einstein's ' finite but unbounded ' space, which cannot be physically imagined, but only expressed mathematically.

However, the main justification for the use of the word ' model ' in the wide sense is the fact, which we are attempting to establish here, that theories of a purely mathematical kind may function in essentially the same way as physically imaginable models, in being capable of suggesting further lines of development in the explanation of the experimental facts. It is sometimes possible to label models unequivocally as ' mechanical ', ' electrical ', ' mathematical ', and so on, but more usually a model will be a mixture of several types. In this use the word ' model ' becomes co-extensive with the word ' hypothesis ', but is more suggestive, since it calls attention to the heuristic properties of the early mechanical models, and asserts that these properties must be found in any hypothesis which is scientifically useful.

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[1] As described for instance in Whitrow's, *The Structure of the Universe*, London, 1950.

II. The second point to be illustrated below by examples from nineteenth-century physics is the relation between models and the natural phenomena which they are used to explain. I shall suggest that *most physicists do not regard models as literal descriptions of nature, but as standing in a relation of analogy to nature.* The word ' analogy ', however, needs some discussion, as it is used in a variety of senses.

In the literature of physics two important meanings of ' analogy ' may be distinguished.

(*a*) It may be said that there is an anology between two branches of physics in virtue of the fact that the same mathematical formalism appears in the theory of both. An example is the analogy between the theory of heat and that of electrostatics, which, as Kelvin first showed,[1] can be described by the same equations if one reads ' temperature ' for ' potential ', ' source of heat ' for ' positive electric charge ', and so on. In other words, the mathematical structure of the two theories is the same, and consequently one theory may be used as a *model* for the other, as the theory of heat, which was already worked out, was used by Kelvin as a model for the field theory of electrostatics, which he was developing for the first time.

(*b*) ' Analogy ' may refer also to a model such as the billiard ball model of gas molecules. In this case the term expresses the relation between billiard balls and gas molecules, and the model itself (the billiard balls), being one of the relata, is an ' analogue ' of the gas molecules. When we say in this way that there is an analogy between a model and certain phenomena of nature, we are in some sense asserting an identity of mathematical structure between the model and nature, as in sense (*a*) we are asserting such an identity between two theories. But here the meaning of the assertion is obscure unless we can show how to determine the ' mathematical structure of nature '. All that the physicist can *certainly* determine about nature are experimental results, usually expressed by measurements, and therefore the assertion of an analogy must mean at least that there are resemblances between these results and the model. The resemblances are in fact correspondences between the observed measurements and certain numbers deduced from the model ; for example, if the appropriate calculations based on the theory of mechanics are made about the energy of colliding billiard balls, we can obtain a numerical value which is the same as that shown on the scale of a thermometer placed in a vessel containing a gas. Several such numerical correspondences obtained under various conditions are sufficient in physics for the assertion of an analogy between the model and the experimental facts, the analogy consisting of the correspondence between numerical consequences of the model, and numerical experimental results. It follows that there may be any number of models of the same set of facts, all yielding similar numbers which agree with experiment.

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[1] *Papers on Electrostatics and Magnetism*, London, 1884, p. 1

These two senses of ' analogy ', to be found in the literature of physics, lead us to adopt the following definition :

> An analogy in physics is a relation, either between two hypotheses, or between a hypothesis and certain experimental results, in which certain aspects of both relata can be described by the same mathematical formalism.

Such a definition is not too far removed from that use of ' analogy ' in modern logic in which it means a relation between entities having some characteristics in common.[1] Thus Mill speaks of an analogy between the moon and the earth, the moon being ' a solid, opaque, nearly spherical substance ; . . . receiving heat and light from the sun, in about the same quantity as our earth ; revolving on its axis ; composed of materials which gravitate, . . .' (op. cit., p. 90). It is convenient for purposes of the logic of physics to limit this use of ' analogy ' as similarity in general, to similarity of mathematical structure.

To say that a model is in general an analogue and not a literal description of nature may in certain cases mean no more than that the model is an idealisation of nature. In classical mechanics, the entities discussed, such as smooth planes, perfectly rigid spheres, and the like, are not literal descriptions of anything to be found in nature, but are simplifications of natural objects arrived at by neglecting all but a few properties of the objects—properties which are selected because they are amenable to mathematical treatment. There is an analogy between a dynamically rigid sphere and a cricket ball in the sense that the same mathematical formalism describes the behaviour of both more or less accurately. But the approximation involved in applying mathematics to nature is not the main point which it is desired to emphasise in calling a model an analogue of nature. The main point becomes significant only when the model is used in theories such as atomic physics where the entities discussed, protons, neutrons, etc., are, unlike cricket balls, observable only in virtue of their remote effects : it is as if the properties of cricket balls were known to us, not by seeing and handling them, but only by hearing a sharp impact as a batsman hits out and observing shattered windows. To speak of *atomic particles* at all is to employ a model based on dynamics and electrostatics, and such a model is not simply an idealisation of something which is observed but is too complicated to deal with as it exists in nature ; but is a hypothesis adopted because deductions from it, sometimes very remote deductions, do yield numbers comparable to the experimental measurements. All that can then be said with certainty is that there is a similarity of mathematical structure between the model and the experiments, in other words, there is a relation of analogy in the sense which has been defined above.

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[1] e.g. Mill, *A System of Logic*, 10th edn., London, 1879, vol. 2, pp. 87 ff. ; Keynes, *A Treatise on Probability*, London, 1921, p. 222

The difference between analogy and literal description can be illustrated by another familiar model in physics : that of two gravitating masses, attracting each other along the line joining them by a force inversely proportional to the square of the distance between them. This fulfils the necessary conditions for a model, in that there is a well-worked-out mathematical theory, the theory of potential, which provides rules of manipulation for the concepts. Now when we attempt to use the gravitational model in a theory about a different range of physical phenomena, say the interaction of the fundamental particles, there are two types of question that can be asked about the relation between the model and the natural phenomena which it describes :

(i) Does matter *really* consist of systems of particles acting on one another according to the inverse square law ?

(ii) Is the behaviour of the fundamental particles more *analogous* to a system of gravitating particles than to anything else with which we are well acquainted ? This, if the relation of analogy is taken to mean similarity of mathematical structure in the sense explained above, means in effect, asking whether the gravitational theory provides the most suitable mathematical language system within which to correlate the behaviour of the fundamental particles.

The distinction between the two types of question can be illustrated by the work of some of the physicists of the last century, and it is instructive to inquire which of the two questions they believed themselves to be answering. Nineteenth-century physics is commonly designated ' mechanist ', by which is usually meant that a question of the type (i) above is to be answered in terms of, to use Larmor's phrase, ' the pressures and thrusts of the engineer, and the stresses and strains in the material structures by which he transmits them from one place to another ',[1] as if matter *really* consisted of tiny Meccano model cogs, gyrostats and spiral springs. Now in fact, judging by the incidental remarks about method which are to be found in the writings of the nineteenth-century mathematical physicists (and they are fairly free with their logical comments upon what they are doing), they were often quite clear about the significance of the mechanical models which they constructed to embody their equations. They knew, in other words, that the question to be asked was question (ii) ; their so-called ' mechanistic ' outlook arose, not from a false metaphysics, but from the fact that they were not yet familiar with mathematical languages other than those of classical mechanics and hydrodynamics, and that they therefore tended to express their hypotheses always in terms of those languages.

2 *Hypothesis in Nineteenth-century Physics*

It would be easy to compile a list of quotations apparently showing that, far from having a naïve faith in the reality of models, many of the great nineteenth-century theorists held a positivist theory of science long before Mach laid down his classic statement of scientific positivism in the 1900s.[2] Fourier, for example, is at pains to point out in the *Théorie analytique de la chaleur* that his equation of the conduction of heat, and all the mathematical results he derives from it, are quite independent of any theory of the physical manner in which heat is communicated from one part of a body to another. All that is necessary is to lay down the principle that the rate of flow of heat between two surfaces of the body is proportional to their difference of temperature ; both rate of heat flow and temperature difference being quantities that can be measured at the surface of a body. The only further assumption involved is that the flow and the temperature are continuous within the body where measurements cannot be made, in other words, that heat is analogous to a continuously flowing liquid. But it is not necessary to enquire further into its nature ; nothing would be added to the mathematical description of the observations by doing so : ' In whatever manner we please to imagine the nature of this element [heat], whether we regard it as a distinct material thing which passes from one part of space to another, or whether we make heat consist simply in the transfer of motion, we shall always arrive at the same equations.' [1]

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[1] *British Association Report*, London, 1900, p. 618

[2] Comte wrote in the 1850s, but he was not himself a scientist, and although he was undoubtedly read by many scientists it is not clear that he had much direct influence on their philosophy of science.

Again, the development of the theory of elastic solids was confused by the molecular hypotheses of the earlier writers. Poisson, assuming definite laws of action between the molecules of a substance, arrived at equations for the stress-strain relationship which implied that the modulus of compression of a solid is always a constant multiple of its rigidity. This holds only for so-called ideal solids where the molecules are displaced regularly with respect to each other under strain. Stokes, on the other hand, always cautious with regard to untestable hypotheses, developed a more adequate theory of elasticity [2] by considering solids to be continuous on the average if the equations are confined to volumes containing many molecules, and by then treating the subject as an extension of the theory of continuous viscous fluids. As in Fourier's theory of heat, the observed phenomena can be explained without entering into further details about exactly how stress is communicated from molecule to molecule. All this seems very similar to modern operationalism, but it will be noticed that in each of the cases quoted above some assumption, namely, of continuity of heat-flow or of stress within material substances, is involved, and the assumption is not a *deduction* from observations—it is properly a hypothesis or model.

3 *Theories of the Luminiferous Aether*

The necessity for hypotheses becomes even clearer when we consider more ambitious theories such as those concerning the transmission of light. It was universally believed that light was transmitted through space as a wave-motion in a material substance called the aether, but this unobservable concept did not cramp the theory as much as we sometimes imagine. The great problem was to see how a substance, having at least some of the properties of matter as we know it, could transmit a wave-motion having the observed properties of light. In the first instance, efforts were directed towards constructing a *mathematical* formalism which would lead to the correct laws of refraction, reflection, polarisation, and the rest, without necessarily describing a corresponding physical mechanism. Green, in a paper of 1838,[1] introduces his theory as follows :

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[1] Fourier, *The Analytical Theory of Heat*, Cambridge, 1878, p. 464

[2] Stokes, *Mathematical and Physical Papers*, I, Cambridge, 1845, p. 75

> . . . we are so perfectly ignorant of the mode of action of the elements of the luminiferous ether on each other, that it would seem a safer method to take some general physical principle as the basis of our reasoning, rather than assume certain modes of action, which, after all, may be widely different from the mechanism employed by nature ; more especially if this principle . . . lead to a much more simple process of calculation.

The principle in question, from which Green proceeds to deduce the equation of motion of the medium and to derive the laws of reflection and refraction, is the principle of stationary action, which in Lagrange's analytical treatment of Newton's mechanics is the starting point from which the laws of mechanics may be deduced. In other words, although Green makes no attempt to describe the precise mechanical action of the aether, he assumes that its action is mechanical in the sense of Newton's theory, and starts from the most general equation consistent with that assumption. His model is not a physically picturable model of colliding billiard balls or vibrating springs, it is the mathematical formalism of Lagrange's analytical mechanics.

The Irish mathematician MacCullagh indulges in even more obvious wresting of the mathematics in order to make it fit the phenomena, irrespective of whether the resulting hypothesis has any physical meaning or not. He is quite explicit about his method ; in a paper on ' A dynamical theory of crystalline reflection and refraction ' [2] he states that his aim is to deduce the laws of propagation and reflection of light in crystals from common principles by the methods of analytical dynamics. He makes one or two limited assumptions about the aether, namely, that its density is constant everywhere, that

[1] Green, *Mathematical Papers*, Paris, 1903, p. 245

[2] MacCullagh, *Trans. Roy. Irish Acad.*, 1848, **21**, 17

it can be regarded on the average as continuous, and that the mutual action of its particles is negligible at distances comparable with a wave-length of light. He then writes down the most general mechanical equation of such a medium in its variational form, derived from the principle of stationary action :

$$\int\left(\frac{\partial^2 u}{\partial t^2}\,\delta u+\frac{\partial^2 v}{\partial t^2}\,\delta v+\frac{\partial^2 w}{\partial t^2}\,\delta w\right)dxdydz=\int\delta V\,dxdydz.$$

Here the left-hand side refers to the kinetic energy of the displaced particles of the medium, and V is their potential energy depending on their mutual actions and reactions. Now no physical assumption has been made about these actions, and MacCullagh proceeds to find a function V such that the equation will represent what is known observationally of the propagation of light waves in crystals. The function turns out to be one having no simple interpretation in terms of the actions of aether particles, but ' having arrived at the value of V, we may now take it for the starting point of our theory, and dismiss the assumptions by which we were conducted to it '. Of the validity of this procedure MacCullagh says :

> In this theory, everything depends on the form of the function V. . . . But the reasoning which has been used to account for the form of the function is indirect, and cannot be regarded as sufficient in a mechanical point of view. It is, however, the only kind of reasoning which we are able to employ, as the constitution of the luminiferous medium is entirely unknown.[1]

This reasoning uses the formalism of analytical mechanics as a model, generalising the function V, which was originally the potential energy calculated from a mechanical model, to be any function admissable in the variational equation. The form of V derived was not in this case merely a convenient mathematical summary of the phenomena which MacCullagh set out to explain, for he later found that it led to the correct equations for a new set of phenomena, those of total reflection. Thus the mathematical formalism functions in all respects like a model which can be thought about and generalised independently of the data it is being used to explain. Such reasoning from mathematical models is common at this period, especially in MacCullagh's work. Another example is the attempt to formulate a theory for the rotation of the plane of polarisation of light when it passes (*a*) through a crystal, and (*b*) through a transparent magnetised body. The equations of rectilinear light propagation in a vacuum are the well-known wave equations :

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[1] ibid., p. 50

$$\frac{\partial^2 Y}{\partial t^2} = c^2 \frac{\partial^2 Y}{\partial x^2}, \quad \frac{\partial^2 Z}{\partial t^2} = c^2 \frac{\partial^2 Z}{\partial x^2},$$

where Y, Z, stand for the components along the y- and z-axes respectively of a periodically varying quantity which is associated with the light and which need not be further defined at the moment. MacCullagh showed[1] that the phenomena observed when light passes through a crystal can be deduced from a modification of the above equations :

$$\frac{\partial^2 Y}{\partial t^2} = c^2 \frac{\partial^2 Y}{\partial x^2} + a \frac{\partial^3 Z}{\partial x^3}, \quad \frac{\partial^2 Z}{\partial t^2} = c^2 \frac{\partial^2 Z}{\partial x^2} - a \frac{\partial^3 Y}{\partial x^3}.$$

Following the same procedure, Airy[2] showed that the behaviour of light passed through a magnetised body can be deduced from

$$\frac{\partial^2 Y}{\partial t^2} = c^2 \frac{\partial^2 Y}{\partial x^2} + b \frac{\partial Z}{\partial t}, \quad \frac{\partial^2 Z}{\partial t^2} = c^2 \frac{\partial^2 Z}{\partial x^2} - b \frac{\partial Y}{\partial t},$$

or any similar pair of equations in which the last term is an odd differential with respect to t, and an even differential with respect to x, the exact form depending on the amount of rotation of the plane of polarisation of the light, which can be determined by experiment. Airy remarks :

> I offer these equations with the same intention with which Prof. MacCullagh's equations were offered ; not as giving a mechanical explanation of the phaenomena, but as showing that the phaenomena may be explained by equations, which equations appear to be such as might possibly be deduced from some plausible mechanical assumption, although no such assumption has yet been made.[3]

4 *Maxwell and Kelvin*

Both MacCullagh and Airy seem to regard this mathematical theorising as inadequate : for them it is only the first step towards a truly mechanical theory. At that stage in the theory of light the mere multiplication of equations was certainly inadequate, because, although they could be generalised to a limited extent to cover closely related types of observation, there was nothing like a complete mathematical theory resting on a few simple axioms from which all light phenomena could be derived. At this time, however, physicists could not conceive of such a general theory unless it were embodied in a mechanical model, and efforts were directed towards constructing ever more complicated mechanical systems which would correspond to the equations arrived at by mathematical reasoning from the observations. But it is fairly clear that these mechanical models were intended only as a guide to thought, they were known to be analogies and not literal representations of the structure of nature. Thus Maxwell's famous vortex theory in which matter (or aether—Maxwell himself was in doubt) consisted of a distribution of vortices along lines of magnetic force, connected by perfect rolling friction with ' idle wheels ' whose linear motion constituted electric current, was not regarded by him as ' a mode of connexion existing in nature ' :

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[1] MacCullagh, *Trans. Roy. Irish Acad.*, 1837, **17**, 461

[2] Airy, *Phil. Mag.*, 1846, **28**, 469

[3] ibid., p. 477

> The attempt . . . to imagine a working model of this mechanism must be taken for no more than it really is, a demonstration that mechanism may be imagined capable of producing a connexion mechanically equivalent to the actual connexion of the parts of the electromagnetic field. The problem of determining the mechanism required to establish a given species of connexion between the motions of the parts of a system always admits of an infinite number of solutions.[1]

Thomson (later Lord Kelvin) was an inveterate model-maker, whose suggestions were sometimes over-ingenious, and sometimes considerable aids to progress. For instance, he compared the aether to a homogeneous air-less foam, having *negative* compressibility (that is, increase of pressure leading to increase of volume), but this model was superfluous, for in the case of the propagation of light through pure aether (' empty ' space) Maxwell's mathematical theory provided all the model required, although Kelvin to the end of his life never reconciled himself to purely mathematical theories in physics. Again, he constructed a model of the type of aether at which MacCullagh had arrived by the mathematical reasoning described above. This aether has the property that the potential energy V depends only on the rotation of the volume elements and not on their compression and distortion. It is difficult to imagine any physical model having such a property, but here is Kelvin's model as described by Whittaker :

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[1] Maxwell, *Treatise on Electricity and Magnetism*, 2, Oxford, 1881, p. 427. Maxwell had an exceptionally clear idea of the function of hypothesis and model in physical theories. The first few pages of his paper ' On Faraday's Lines of Force ' (*Scientific Papers*, 1, p. 155) are worth studying from this point of view, but the relevant passages are too long for quotation here. H. Poincaré showed in detail in the introduction to his *Electricité et Optique* (Paris, 1890) that if functions T (the kinetic energy) and V (the potential energy) can be derived from the observed co-ordinates of a system, such that they satisfy the principles of the conservation of energy and of least action, then there is always an infinite number of mechanisms which would reproduce the phenomena as observed.

> Suppose . . . that a structure is formed of spheres, each sphere being in the centre of a tetrahedron formed by its four nearest neighbours. Let each sphere be joined to these four neighbours by rigid bars, which have spherical caps at their ends so as to slide freely on the spheres. . . . Now attach to each bar a pair of gyroscopically-mounted flywheels, rotating with equal and opposite angular velocities, and having their axes in the line of the bar ; . . . the structure as a whole will possess that kind of quasi-elasticity which was first imagined by MacCullagh.[1]

Of course, neither Kelvin nor anyone else believed that such contraptions pervaded all space from the interior of molecules to the furthest stars. Kelvin's own attitude towards the significance of these models is shown by his discussion of molecular models in his *Baltimore Lectures*.[2] Here he constructed models to illustrate the interaction of aether and matter, and they were more than amusing exercises in the interpretation of equations—they were in fact valuable stepping-stones to the electron theory of matter which was developed at the end of the century, and so to the atomic models still used in modern quantum physics. It is interesting to see how, towards the end of the century, electrical models gradually took the place of mechanical ones even in Kelvin's work, a process discernible in the *Baltimore Lectures* between the time they were delivered in 1884 and finally published in amended form in 1904. The main object of these lectures was to discuss various theories of anomalous dispersion of light, involving models of the luminiferous aether. Dispersion is the change in velocity of light, different for different wave-lengths, which takes place when light is passed through certain material media, and it is most reasonably explained as the result of natural frequencies of vibration of the molecules of the matter itself interacting with the vibrating aether particles, or whatever constitutes the light wave. Kelvin accordingly proposes (in 1884) what he calls a 'crude mechanical model' of molecules, consisting of concentric vibrating spheres of different densities connected by springs to each other and to the rigid lining of an ideal spherical cavity in the aether. He shows that such a model will account for the dispersion phenomena, and remarks that it could be put 'not in a rude mechanical model form, but in a form which would commend itself to our judgement as presenting the actual mode of action of the particles of gross matter whatever they may be upon the luminiferous ether'.[1] At the end of the published form of the lectures in 1904, this dynamical model is replaced by the hypothesis of electrical attraction of an atom on its 'electrion' (charged particle) when the latter is displaced from the centre of the atom, and of an attraction between the electrion and the surrounding aether proportional to their relative acceleration. Kelvin remarks :

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[1] Whittaker, *History of the Theories of Aether and Electricity*, Edinburgh, 1951, p. 145. This history does not itself attempt to analyse methodology, but is an invaluable source-book for the logician of nineteenth-century physics.

[2] W. Thomson, *Baltimore Lectures*, Cambridge, 1904

> It is interesting to see that every one of the formulas . . . are applicable to both the old and the new subjects : and to know that the solution of the problem in terms of periods is the same in the two cases, notwithstanding the vast difference between the artificial and unreal details of the mechanism thought of and illustrated by models in 1884, and the probably real details of ether, electricity and ponderable matter, suggested in 1900-1903.[2]

5 *Summary*

The conclusions to be drawn from this discussion of the significance of models in nineteenth-century physics may be summarised as follows :

(*a*) The types of models used are very varied—there are purely mechanical structures containing a whole workshop full of balls, rods springs, and flywheels ; there are continuous elastic solids ; there are vortices and so on drawn from the theory of hydrodynamics ; and there is the mathematical formalism of dynamics itself functioning a as model.

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[1] W. Thomson, *Baltimore Lectures,* Cambridge, 1904, p. 13

[2] ibid., p. 467

(*b*) In many cases the real progress was made in terms of a mathematical model: the mechanical model was then added only as an afterthought in the mistaken belief that it endowed the mathematics with a respectability it would not otherwise possess.

(*c*) On the whole, and especially by Maxwell, models were understood to be analogues and not literal descriptions of nature.

6 *Characteristics of Models*

We are now in a position to attempt a definition of ' model ' as it is used in physics. The examples given above make it clear that such a definition must be general enough to include mathematical formalisms as well as mechanical or other physically imaginable models. Models have two sorts of characteristics which may be called respectively *formal rules* and *pointers*, and a description of these will serve as a partial definition.

Formal rules. The model has a necessary internal structure which may consist simply of the axioms and rules of inference of a mathematical formalism, as in the case of mathematical models, or which may have in addition a number of axioms suggested by the empirical laws of some physical process, as in the case of mechanical or electrical models. For example, if a simple pendulum is introduced into a mechanical model, this is equivalent to adding to the model the axiom ' the law of motion of this part of the model is $\ddot{x} + n^2x = 0$ '. The law is empirical if it refers to an actual, physical, pendulum, but the model in this case is an idealisation of the physical process, in which the empirical laws of the process are regarded, for the purposes of this particular model, as axioms of a deductive system. This is why a great deal of modern mathematical physics has the appearance of a purely deductive system, for much of it is concerned with working out the formal consequences of a model whose mathematical and physical characteristics are for the time being assumed as axioms.

Pointers. A model is not simply a system of formal rules, for it carries with it suggestions for its own extension and generalisation. If any formal system is looked at as one possible set of axioms and rules of inference, selected out of an infinite number of possible, and internally consistent, sets, then there will be certain other systems which can be reached from the first system by making simple generalisations and additions in the axioms of the first system. In the case of a mathematical model, whose formal rules may have no immediate physical interpretation, these generalisations and modifications will be suggested by the mathematics itself, just as the function V in MacCullagh's equations started as a physical potential and was generalised to mean any function mathematically admissable in the equations. In the case of a mechanical model the formal rules have been derived from the empirical behaviour of some mechanical process, and additions to these rules will be suggested by taking account of certain features of the empirical process which were previously neglected. For example, the elastic sphere model of gas molecules is derived from the rigid billiard ball model by taking account of the fact that actual bodies are not perfectly rigid, and adding the appropriate equations of elasticity to the formal rules of billiard ball motion.

The pointers are contained in a haze of mathematical and physical associations surrounding the model, some of which will be misleading and some of which will be useful for further progress. The obvious pointers to follow in any given case seem to be those which suggest new formal rules which are easily worked out in terms similar to those of the original set. For instance, it would not be helpful to extend the billiard ball model of molecules by taking account of the fact that actual billiard balls have some colour, because colour is not one of the concepts which occur in the theory of colliding bodies ; in fact, the laws of motion of black billiard balls are the same as those of balls which have the same size, shape and mass, but which happen to be red. Elasticity on the other hand, is a concept which does occur in the theory of collisions and is therefore a suitable candidate for a more general theory of gas molecules. But it is not always easy to see without further experiment which characteristics of a model can be exploited in a more general theory and which are irrelevant and misleading. For example, it was not possible to see before the Michelson-Morley experiment that the properties of the medium in which the waves moved were irrelevant to the wave model of electromagnetic phenomena. Speculation upon the nature of this medium would, and did, seem a natural way of attempting to extend the wave model until further experiment showed the phenomena to be consistent with some formal aspects of the wave theory, but not with the assumption of a medium having the properties of any known material substance. There is bound to be a certain haziness about the pointers of a model ; they may suggest any one of an indefinite number of modifications of the formal rules of a model, and the process of picking out relevant and useful ones is precisely the process of scientific research, for which no rules can be given.

Not only has each model an indefinite number of pointers, but since there is an indefinite number of ways of adding to the mathematical structure which forms the basis of the analogy, there is also an indefinite number of different models of any given physical situation, each having the same set of formal rules, but having different pointers, some of which may contradict the others. The particle and wave models of light have sets of formal rules which are analogous to each other and to certain elementary properties of light, but the particle model points to further properties, for example, atomicity and rectilinear propagation, which, without further elaboration of both models such as has occurred in the quantum theory, contradict properties such as interference and diffraction, which are suggested by the wave model. In practice the simplest and most familiar model will be the one that is tried first, but what is still left entirely mysterious by this account of the use of models in physics, is the fact that simple and familiar models are so successful so often. Short of some metaphysical postulate of the unity of nature there is no *a priori* reason why light should behave in the least like particles or waves, or why the fundamental particles (even the name indicates how far analogy permeates our thinking) should behave like gravitating planets or electrified pith-balls, or indeed in any way that can be described by existing mathematical theories. We ought to be prepared for, rather than surprised at, the inadequacy of familiar models in much of modern physics.

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WHITEHEAD'S METHOD OF EXTENSIVE ABSTRACTION

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1 *Introduction*

SEVERAL considerations led A. N. Whitehead to develop his method of extensive abstraction : [1] (*a*) The theory of relativity has abandoned the conception of space as a structured aggregate of absolute positions, serving only accidentally and indifferently as a receptacle or container of matter. On the relational theory of space demanded by relativity, geometry is the science not of absolute 'container'-space, but of the complex relations obtaining directly between *physical things*.[2] (*b*) If science is to be 'founded on observation',[3] all physical entities 'must be defined in terms of genuine natural entities', that is in terms of 'What sense-awareness delivers over for knowledge'.[4] Thus, all 'The constructions of science are merely expositions of the characters of things perceived'.[5] (*c*) Since points and such other entities as have the same formal properties (for example, elements of time devoid of temporal extension, and 'event-particles' or 'instantaneous point-flashes') are not among 'the immediate data of perception',[6] the points of Euclid's geometry and the mass-points and point-events of mechanics are *not* 'genuine natural entities'. (*d*) We must therefore abandon the nineteenth-century approach to the foundations of geometry, an approach which 'proceeded from the assumption of points as ultimate given entities'.[7] And we must 'describe what a point is' by showing 'how the geometric relations between points issue from the ultimate relations between the ultimate things which are the immediate objects of knowledge'.[8]

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[1] A. N. Whitehead, 'La Théorie Relationniste de l'Espace', *Rev. de Métaph. et de Morale,* 1916, **23**, 423 ; *An Enquiry Concerning the Principles of Natural Knowledge* (referred to below as *PNK*), Cambridge, 1919, Part 3 ; *The Concept of Nature* (referred to below as *CN*), Cambridge, 1926, Ch. 4 ; and *Process and Reality* (referred to below as *PR*), New York, 1929, Part 4.

[2] *PNK*, p. 5

[3] *CN*, p. 57

[4] *idem*

[5] *CN*, p. 148

[6] *PNK*, p. 5

[7] *idem*

[8] *idem*

If Whitehead is to implement this programme successfully, points must satisfy two conditions, which C. D. Broad expressed succinctly as follows :[1]

> (1) . . . points must have to each other the kind of relations which geometry demands ; and (2) . . . points must have to finite areas and volumes such a relation that a reasonable sense can be given to the statement that such areas and volumes can be exhaustively analysed into sets of points.

It is the aim of the present paper to show that Whitehead's Method of Extensive Abstraction fails to achieve its stated objective and that all such positivistic constructive attempts must share that failure. All but one of the mathematical and logical arguments which I shall urge against Whitehead's Method seem to have been neglected in earlier critiques by other authors.[2] Specifically, I shall argue (i) that the Method is vitiated by Zeno's mathematical paradox of plurality because of an important violation of the two conditions stated by Broad ; (ii) that the convergence of the Method's abstractive classes is fatally ambiguous ; (iii) that Whitehead's modification of the Method in *Process and Reality* does not remove this crucial ambiguity ; (iv) that the abstractive classes do not belong to the domain of sense awareness ; and (v) that Professor A. P. Ushenko's recent defence of the Method[3] is unsatisfactory.

2 *Whitehead's Method and Zeno's Mathematical Paradox of Plurality*

According to several writers,[4] the purpose of one of Zeno's paradoxes of plurality was to show that since spatial and temporal intervals are extended, it is self-contradictory to regard them as aggregates respectively of unextended points and instants. Georg Cantor's ardent advocacy of point-set theory as a foundation shaped the construction of much of twentieth-century mathematics, for instance, of the topological theory of dimension.[1] And thus part of the edifice of contemporary mathematics rests precisely on the conception, proscribed by Zeno, that an interval is literally composed of unextended point-elements. I have explained elsewhere[2] that since the measure of every denumerable point-set is zero,[3] an interval *can* be consistently regarded as an aggregate of points only if this aggregate is *super-denumerably* infinite. Hence, to insure freedom from the contradiction which Zeno exposed in the point-set geometries of his time, an epistemological reconstruction of modern geometry must succeed in preserving the super-denumerability of intervals. Accordingly, we ask : Can Whitehead establish the existence in sensed nature of the super-denumerable infinity of abstractive sets required by intervals for metrical consistency in the context of his Method ? The answer could be in the affirmative only if a super-denumerable infinity of objects could be among what Whitehead calls ' the termini of sense-awareness '.[4]

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[1] C. D. Broad, *Scientific Thought,* London, 1923, p. 39

[2] The critical analysis of the ontology of Whitehead's Method offered in N. Lawrence's careful recent study (*Philos. of Science,* 1950, **17,** 142) was 'not intended as an indication of its [the Method's] failure'.

[3] A. P. Ushenko, 'Einstein's Influence on Philosophy', in *Albert Einstein : Philosopher-Scientist,* ed. P. A. Schilpp, Evanston, 1949, pp. 632-645, esp. p. 634 n

[4] cf. H. Hasse and H. Scholz, *Die Grundlagenkrisis der griechischen Mathematik,* Charlottenburg, 1928, p. 11 ; S. Luria, 'Die Infinitesimaltheorie der antiken Atomisten', *Quellen u. Studien zur Gesch. der Math., Astr., u. Phys.,* 1933, Abt. B (Studien), **2,** 106-107 ; P. Tannery, 'Le Concept Scientifique du Continu : Zénon d'Elée et Georg Cantor', *Rev. Phil.,* 1885, **20,** No. 2, 385-410

Empiricists from Aristotle to Hume have maintained that *infinitum actu non datur.* But let us suppose for the sake of argument that, contrary to that tradition, the existence of a *denumerable* actual infinite were somehow certifiable by sense awareness so that the meaning of aleph zero could still be given a sensationist pedigree. It would then nevertheless be true that the very notion of actually infinite classes having a cardinality *exceeding* aleph zero would inexorably defy encompassment by the sensory imagination. For the set-theoretical meaning of super-denumerability eludes all logically possible *sensory* exemplification, since *any* collection of non-overlapping three-dimensional regions of space is at most *denumerably* infinite (cf. G. Cantor, *Math. Ann.*, 1882, **20,** 117).

Thus the manner in which *the super-denumerable kind of infinity* is conceived[5] shows clearly that sense-awareness cannot suggest the idea of a collection of perceptible regions whose cardinality exceeds $\aleph_0$, and, more significantly, that *a fortiori*, sense awareness *cannot exhibit the actual existence* of such a collection in sensed nature. And, on Whitehead's assumptions, it is the *latter* condition which must be satisfied, if Cantor's *actual* super-denumerable infinities of points, which are required by modern geometry and physics for metrical consistency, are to be preserved through epistemological reconstruction. For the existence of points allegedly derives from and is exhausted by the existence in sensed nature of the abstractive classes defining them. It follows inescapably that the Method is rendered a failure merely by the exigencies of metrical consistency and that it cannot validly serve Whitehead's purpose, which Hammerschmidt[1] aptly characterised as discarding the classical idea of a continuum as a mere closely packed collection of discrete points and substituting for it the conception of the continuum as an exhibition of the interconnectedness of regions, derived from their basic extensiveness.

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[1] cf. W. Hurewicz and H. Wallman, *Dimension Theory,* Princeton, 1941

[2] A. Grünbaum, ' A Consistent Conception of the Extended Linear Continuum as an Aggregate of Unextended Elements ', *Philos. of Science*, Oct. 1952, **19,** § 6

[3] ibid., § 4

[4] Among other places, this designation is used by Whitehead in *CN*, p. 92.

[5] For details on the set-theoretical reasoning in the diagonal method, see Georg Cantor, *Gesammelte Abhandlungen*, ed. E. Zermelo, Berlin, 1932, p. 279.

As N. Lawrence has shown,[2] Whitehead is far from single-minded in his stand on whether the points of mathematical physics as such must be individually endowed with physical existence. Thus we find Whitehead saying, on the one hand, that 'you must not think of the world as ultimately built up of event-particles'.[3] And yet elsewhere,[4] he says that 'when we speak of a point in the timeless space of physical science, I suppose that we are speaking of something in nature. If we are not so speaking, our scientists are exercising their wits in the realms of pure fantasy, and this is palpably not the case.' There are important reasons why this latter view of the rôle of points ought to be accepted and should then be used as a basis for assessing the epistemological import of the failure of Whitehead's Method to avoid the Zenonian metrical paradox: (*a*) The equation $x = vt$, describing the uniform motion of a mass-point along the x-axis, affirms a bi-unique correspondence between instants of time t and points x on the path occupied by the moving point. Accordingly, physical theory treats the motion as an aggregate of *point-events* or coincidences of the moving point with points x on the path. (*b*) G. Cantor had the support of such modern scientists as Faraday, Ampère, W. Weber, and Cauchy, when he wrote that 'in order to achieve a more satisfactory *description of nature*, the ultimate or genuinely *simple* elements of matter must be postulated as *actually infinite in number and . . . spatially* they must be regarded as *entirely without extension*'.[5] (*c*) The theoretical measurables of physics range over a mathematically continuous domain and their values are theoretically determined by real number co-ordinates of *points* on a scale. Since points have this fundamental importance in mathematical physics, the failure of Whitehead's Method to cope with them suffices to disqualify the epistemological axioms of that method as a basis adequate to a rational reconstruction of science. An elaboration of a satisfactory characterisation of the physical status of points on an *alternative* epistemological basis must await another occasion.

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[1] W. W. Hammerschmidt, *Whitehead's Philosophy of Time*, New York, 1947, p. 43

[2] N. Lawrence, op. cit.

[3] *CN*, p. 172

[4] *CN*, p. 96

[5] Cantor, op. cit., p. 275

3 *Abstractive Classes and Ambiguity of Convergence*

The Dedekind-Russell definition of irrationals would be rejected as fatally ambiguous, if, contrary to fact, it were such that it did not provide decisively for a clear differentiation between *every* two irrationals, however close together these may be in magnitude. Precisely that kind of difficulty does, however, beset Whitehead's Method for points. For consider two distinct neighbouring points such as $x = 0$ and $x = 10^{-1000}$. These points differ in identity so much as to be separated by a continuum of other points. And hence we ask : Do we know from sense perception that there exist two *different* abstractive classes defining these two points and, if so, precisely how is their particular difference certifiable by sense perception ? Hume has shown[1] that the structure of appearance is irremediably non-isomorphic with that which geometry attributes to space in virtue of the infinite divisibility of intervals. And precisely because of this lack of isomorphism, sense perception itself provides no means for distinguishing from one another the abstractive classes which Whitehead needs to confer a separate identity upon such points as $x = 0$ and $x = 10^{-1000}$. The difficulty here is intrinsic to the positivistic assumptions on which Whitehead's Method rests and issues in Whitehead's failure to provide for distinguishing a given point from a continuum of others ! For the Method is inherently incapable of ascertaining meaningfully a difference in identity between the class of sensible volumes defining a given point and the class defining another point closely nearby, although the two points are separated from one another by a superdenumerable infinity of intervening points. And thus the Method obliterates the very precision of meaning made possible in the statement of physical laws by the use of real variables.

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[1] D. Hume, *A Treatise of Human Nature*, ed. L. A. Selby-Bigge, Oxford, 1888, Part 2, section 1. See also N. Goodman, *The Structure of Appearance*, Cambridge, 1951, pp. 304-306.

It would not do to counter this objection by maintaining that while there is an ambiguity of convergence as between the classes of perceptible regions themselves, the numbers representing the volumes of their members in each case do converge to zero in the precise mathematical sense of the limit. Thus it may be mentioned that in the case of the point (0, 0, 0), the volume numbers $\frac{4}{3}\pi r^3$ of the members of the abstractive class of spheres $x^2 + y^2 + z^2 = r^2$ (where r is the radius parameter) do converge to zero. This counter-objection is neither cogent nor relevant, because (*a*) the precise numbers $\frac{4}{3}\pi r^3$ cannot be assigned to any of the crude perceptible regions of Whitehead's ' nature ' but only to the exact spheres $x^2 + y^2 + z^2 = r^2$ of co-ordinate geometry. And these *bona fide* spheres are themselves first defined by means of *points* of analytic geometry whose co-ordinates already presuppose the point (0, 0, 0). Hence the spheres of co-ordinate geometry cannot be invoked here to define the point (0, 0, 0) without begging the question, and the convergence of the volume numbers to zero is unavailing. (*b*) The convergence of the volume numbers of the members of the several abstractive classes cannot provide the criterion required for *differentiating* the abstractive classes corresponding to two different (nearby) points. For the sets of volume numbers corresponding to both of these classes *alike* converge to zero.

I conclude that Whitehead's Method does not make provision for the difference in identity between points in systems of mathematical physics.

4 *The Modification of the Method in ' Process and Reality '*

It behoves us to see whether Whitehead's modification of the Method in *Process and Reality* [1] succeeds in eliminating the ambiguity of convergence charged against the Method in the preceding section.

There is good textual evidence, as Mr. Abner Shimony has pointed out to me, that in *PR*, Whitehead no longer requires the members of abstractive classes to be *sensed* regions. Hence, my criticisms are not aimed at the rather baffling treatment in *PR*. But they *are* directed against conceptions of the Method which (*a*) require with Broad that the Method rest on *sensationist* foundations, as in Whitehead's earlier books, and (*b*) would invoke the revised definition of abstractive sets given in *PR* in an attempt to avoid ambiguity of convergence.

[1] *PR*, Part 4, Ch. 2, esp. pp. 450, 454

His earlier definition of 'abstractive class' stated :[1]

> A set of events is called an 'abstractive class' when (i) of any two of its members one extends over the other, and (ii) there is no event which is extended over by every event of the set.

In response to critical suggestions by T. de Laguna, Whitehead modified his definitional edifice in *Process and Reality* where he introduced the following definitions :[2]

> *Definition* 9. A region B is 'non-tangentially' included in a region A when (i) B is included in A, and (ii) there is no third region which is externally connected with both A and B. . . .
>
> *Definition* 10. A set of regions is called an 'abstractive set', when (i) any two members of the set are such that one of them includes the other non-tangentially, (ii) there is no region included in every member of the set.

Condition (ii) in both the original definition and in *Definition* 10 guarantees that there is no last, smallest region in the set and thereby endows the set with infinitely many members. But condition (i) in *Definition* 10 supersedes its earlier counterpart by involving the relation of 'non-tangential inclusion'. Whitehead clarifies the nature of this relation by reference to the diagram reproduced here[3] and writes :[4] 'Non-tangential inclusion is illustrated by diagram (i) . . . ; and the two cases—as yet undiscriminated—of tangential inclusion are illustrated by diagrams (ii) and (iii).'

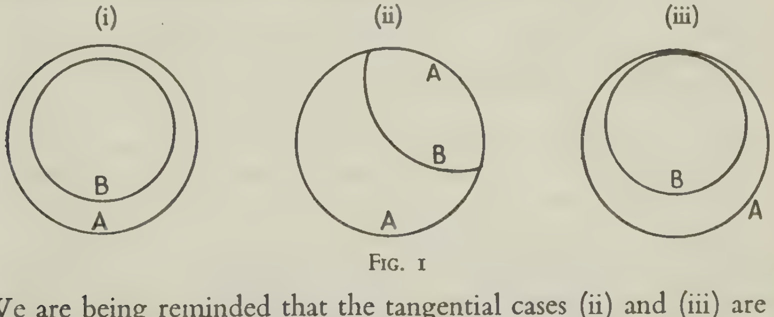

FIG. 1

We are being reminded that the tangential cases (ii) and (iii) are 'as yet undiscriminated'. To discriminate them from one another, Whitehead would have to make use of the notions of point and line. And to do so *at this juncture* would be to commit a glaring *petitio*. For 'we have not defined either points, or lines, or areas ; and . . . we propose to define them in terms of abstractive sets. Thus we must define the various types of abstractive sets without reference to the notions, point, line, area.'[1]

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[1] *PNK*, p. 104

[2] *PR*, p. 454

[3] This diagram is found in *PR*, p. 450

[4] *PR*, p. 454

Not being able now to distinguish, without begging the question, between sets of regions which 'converge upon a line' (case (ii)) and sets whose convergence upon a 'point' has the character of case (iii), Whitehead endeavours to *preclude the ambiguity of convergence shown in case (ii)* by barring both case (ii) and case (iii) from qualifying as 'abstractive sets'. This exclusion of these two cases has the effect of preventing any two members of an abstractive set from having a common boundary.

The issue before us is whether Whitehead's abstractive sets *as now defined* converge to a unique point, thereby nullifying the criticisms of the Method offered in the preceding section of this paper. To see that these criticisms stand despite revision in *Process and Reality* of the definition of 'abstractive set', we note that my charge of ambiguity of convergence was brought against case (i) and is therefore entirely unaffected by Whitehead's attempt, however successful, to eliminate non-uniqueness of convergence in case (ii).

5 *Abstractive Classes and Sense Perception*

A. P. Ushenko, whose defence of Whitehead's Method we shall examine critically below, has observed[2] that 'the usual criticism [of the Method of Extensive Abstraction] is that a geometrical "element", constructed by means of the method, is not observable because it contains an infinite number of regions'.[3] Whitehead himself concedes non-observability here by declaring that entities 'in their capacity as infinite aggregates cannot be the termini of sense-awareness '.[1] And upon remarking that the absence of a last, smallest member of an abstractive set of regions ' has no counterpart in observed nature ', N. Lawrence says : 'Whitehead nowhere tells us what in [sensed] nature forces the conviction on us that there is no last member to series of events in an abstractive set.'[2] Any puzzlement we may harbour as to the reasons for Whitehead's silence on this point is rapidly dispelled by Hume's observation that owing to the existence of a threshold of (spatial) perception, ' no finite [sensed] extension is capable of containing an infinite number of parts '.[3] Inviting us to consider a grain of sand,[4] Hume shows by reference to it that the restriction to a finite number of parts is intrinsic to sensory meanings and does *not* derive from focusing on the content of accidental *de facto* perceptions which might readily be transcended by the sensory imagination.

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[1] *PR*, p. 454. It is very noteworthy that precisely such a programme of definition was rejected at one time by Bertrand Russell in his *Principles of Mathematics* (New York, 1903), where he wrote (p. 443): 'And against any view which starts from volumes or surfaces, or indeed anything except points and straight lines, we may urge, with Peano [the reference given here is *Riv. di Mat.* 4, p. 53] that the distinction between curves, surfaces, and volumes, is only to be effected by means of the straight line, and requires, even then, the most elaborate developments.' But in his later writings (*Our Knowledge of the External World*, London, 1914, pp. 147, 132, 131 and *The Analysis of Matter*, London, 1927, Ch. 28), Russell reversed himself and endorsed Whitehead's Method. One wonders in what way Russell then thought Whitehead had succeeded in overcoming Peano's objections.

[2] Ushenko, op. cit., p. 634, n. 34

[3] We saw in section 4 that condition (ii) in the definition of 'abstractive set' rules out a last, smallest member of the set and thereby requires an infinite membership for all abstractive sets.

How then do Whitehead and the exponents of his Method propose to overcome the difficulty that the infinitudes of regions needed by the Method do not belong to the domain of sense awareness ? To appraise Whitehead's answer, we note first that all the properties of abstractive sets which are essential to the Method are *subsensory*. As he puts it :[5] ' . . . it is the infinite series as it stretches away in unending succession towards the small end, which is of importance. The arbitrarily large event with which the series starts has no importance at all. We can arbitrarily exclude any set of events at the big end of an abstractive set without the loss of any important property to the set as thus modified.' Having said this, he attempts to guarantee the existence of the infinite series of subsensory terms by simply claiming continuity of inclusion among the members of abstractive sets : ' . . . every event contains other events as parts of itself'.[6] C. D. Broad endorses this contention concerning abstractive sets by saying of them : ' . . . ordinary perception makes us acquainted with their earlier and bigger terms, and the assumption that Space is continuous guarantees the later ones '.[7] Whitehead has given no reason why we should not regard Hume's characterisation of *the structure of*

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[1] *CN*, p. 92 [2] N. Lawrence, op. cit., p. 159 [3] Hume, op. cit., p. 30

[4] ibid., pp. 26-27 [5] *CN*, pp. 81-82

[6] *CN*, p. 76. cf. Kant's doctrine that every part of space or time is itself a space- or time-interval having parts (Immanuel Kant, *Critique of Pure Reason*, trans. N. K. Smith, London, 1929, pp. 203-204).

[7] Broad, op. cit., p. 44

appearance as undeniably correct. Thus the argument from the continuity of inclusion derives its semblance of plausibility wholly from a tacit appeal to the infinite divisibility which geo-chronometry attributes to *physical* space-time (but not to sensed space-time !). Moreover, Broad is begging the question, if his ' assumption that Space is continuous ' is to be granted on the strength of the Cantorean continuity affirmed by geo-chronometry. For *that* continuity is defined on the basis of relations between non-perceptual point-elements which are postulated *ab initio*.

In the absence of a satisfactory vindication by either Whitehead or Broad of the principle of continuity of inclusion, which is required by condition (ii) in the definition of abstractive sets, we shall now consider Ushenko's recent defence of that principle. Ushenko's argument is offered as part of his critique of Einstein's conception of the logic of science.

6 *Whitehead's Method and Einstein's Conception of Science*

Ushenko writes :[1]

> . . . Einstein rejects the ' idea that the fundamental concepts and postulates of physics were not in the logical sense free inventions of the human mind but could be deduced from experience by " abstraction " —that is by logical means '[2] . . . his idea of correlation . . . is not correlation in Russell's sense of logical construction. . . .
>
> I believe Einstein is wrong when he asserts that such scientific concepts as points, . . . , cannot be ' deduced from experience by " abstraction "—that is to say by logical means '. The method of extensive abstraction (in the development and use of which Russell has followed Whitehead) can do just that.

To justify his repudiation of Einstein's view in favour of Whitehead's Method, Ushenko advances the following argument :[3]

> As far as I know no one has attempted to show that the method of extensive abstraction involves some logical or technical error. The usual criticism is that a geometrical ' element ', constructed by means of the method, is not observable because it contains an infinite number of regions. This criticism is invalid. For the infinite number (of regions) is allowed not because it is observable but because it is a

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[1] Ushenko, op. cit., pp. 632-633

[2] Quoted from Einstein, *The World as I See It*, New York, 1934, p. 35.

[3] ibid., p. 634, n. 34

> legitimate concept of formal mathematics. Accordingly, the method does not require an actual exhibition of *all* regions that constitute a point; the requirement is that *any* constituent region should be, in principle, observable. In fact a weaker condition is sufficient in order to provide the construction of a point with an empirical basis. This is to say that identification of a particular region or member of a co-punctual group, i.e. within a group that forms a point by definition, is not required. All that we really need is an illustration of the *kind* of thing that the definition is about. . . . The difference between the illustration and the abstractive set cannot, and need not, be illustrated because of being fully accounted for in terms of pure logic and mathematics. . . . the fact that a total co-punctual group cannot be perceptually given is immaterial in a concern with meaning, i.e. in so far as the proposed definition of a point is meaningful. . . . For even if precision of enclosure cannot be exemplified except through some such configuration as a set of concentric circles, the definition to be understood and empirically justified, may need illustration but does not require exemplification. In other words, Whitehead's definition is a conceptual or logical construction. . . .

Disregarding exegetical problems of Whitehead-interpretation, I shall now endeavour to show why Ushenko's argument on behalf of the Method of Extensive Abstraction is quite unconvincing.

We saw in section 2 that physical theory makes *existential* assertions about point-events and mass-points. Whitehead's Method can successfully reconstruct *these* assertions only if it can justify Broad's contention that 'the volumes and the series of volumes that define points exist quite literally'.[1] But it now turns out that Broad's claim is rendered gratuitous on both the stronger and weaker of Ushenko's requirements for the success of Whitehead's Method. For the laws and facts of physics guarantee the failure of Ushenko's stronger condition, which demands that '*any* constituent region should be in principle observable'. This becomes evident when we consider the nomological possibility of observing regions having volumes of the order of a (small) fraction of an electron. Hence all that remains on Ushenko's stronger condition is a futile logical possibility of observation entirely devoid of the required existential import. And Ushenko's weaker criterion is equally unavailing here, since, at best, it can only indicate *what is meant* by a physical point but cannot provide the *exemplification* which *is* required to underwrite *existential* assertions about points.

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[1] Broad, op. cit., p. 51

This existential difficulty makes itself felt in another way, when Ushenko has recourse to the concepts of pure logic and mathematics. He says: '. . . the infinite number (of regions) is allowed not because it is observable but because it is a legitimate concept of formal mathematics'. If so, then he must tell us in what sense we are to interpret the axiom of infinity, which we need for (one of) Peano's postulates and hence for the theory of real numbers and points. Shall we interpret that axiom as affirming that for every cardinal number, there is at least one set of *perceptible* elements that has it? In that case, Ushenko is confronted with Hume's correct denial, which Ushenko would apparently endorse. And if, on the other hand, mere objects of thought are to populate the infinite sets required by the axiom, then we find again that on Whitehead's premisses, as interpreted by Broad, there is no reason for saying that point-events actually occur or *exist*.

Finally, if the Method of Extensive Abstraction is to 'provide the construction of a point with an empirical basis', not in the sense of Broad's interpretation of Whitehead but in the loose sense of Ushenko's weaker requirement, then one wonders (*a*) how Ushenko conceives that the Method excludes any but the most extremely aprioristic interpretation of the basis of our knowledge of points, (*b*) how the Method can be held to offer a novel and distinctive philosophical interpretation of the status of points, and (*c*) in what sense and on what grounds Ushenko thinks the Method deviates from Einstein's view of points, which Ushenko is anxious to reject.

Ushenko's defence fails; Whitehead's modification in *PR* cannot be used to meet the fundamental difficulty of ambiguity of convergence. Thus defences of his doctrine fail. The doctrine itself fails, because (*a*) in the context of modern geometry, it requires the notion of a non-denumerable infinity of *perceivables*, a conception which was shown to be untenable, (*b*) it obliterates the precision of meaning made possible in the statement of physical laws by the use of real variables, (*c*) its own epistemological assumptions render gratuitous, if not meaningless, the postulate of the continuity of inclusion among the members of abstractive sets, a postulate essential to the Method.

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NOTES AND COMMENTS

A Note on Categories

In *The Concept of Mind* Ryle shows that there are no processes corresponding to the words ' see ' and ' hear ' in the way that there certainly are processes corresponding to the words ' look ' and ' listen '. He draws attention, among other things, to the fact that certain adverbs will not go into the sentence frames ' I see ——' and ' I hear ——' but will go into the sentence frames ' I look ' and ' I listen '. Thus you can look hard and carefully but not see hard and carefully (or not hard and carefully) ; you can listen hard and carefully but not hear hard and carefully (or not hard and carefully). In this sort of way Ryle puts expressions into different types or categories. For example, task words like ' run ', ' look ', ' listen ' are separated off from achievement words like ' win ', ' see ', ' hear '. The test briefly is this : ' A ' and ' B ' belong to different categories if a sentence frame can be found such that if ' A ' is put into its blank the sentence makes sense, and if ' B ' is put into the blank the sentence makes nonsense. It is important to note that the fact that two expressions ' A ' and ' B ' *will* go into the same blank in a sentence frame does *not* prove that they are of the *same* category. This is because the systematic ambiguity of crucial expressions in a sentence frame allows the sentence frame to adjust itself to diverse fillings. For example, ' Miller ' and ' Australia ' (or ' the Australian team ') are of indisputably different categories, for while ' Miller was caught and bowled by Brown ' makes sense and ' Australia was caught and bowled by Brown ' does not. Nevertheless, many sentence frames such as '—— scored another run ', can be found in which both ' Miller ' and ' Australia ' will go. So our category test is one which will show expressions to be of *different* categories but will never conclusively show that they are of the *same* category.

It is worth while to consider whether this test, if pushed to the limit, may not show every expression to be of a different logical category from every other. (In which case we should be wise not to take it too seriously.) Thus ' the seat of the —— is hard ' works if ' chair ' or ' bench ' is put into the blank, but not if ' table ' or ' bed ' is. And if furniture words do not form a category, we may well ask what do. Again most logicians regard names of integers as of the same logical type, but it is easy to show that according to Ryle's test they are not. For example, '1', '2', '3', . . . and '−1', '−2', '−3', . . . all go into the blank of '$\frac{7}{(\ )} = \frac{9}{2}$', and yield either an arithmetical truth or a falsehood. But '0' will not go into the blank. We might say that '0' is different and put it in a category by itself. But then every number is different. Thus '7' will not go into an expression of the form '$\frac{x}{7-(\ )}$', '8' will not go into one of the form '$\frac{x}{8-(\ )}$', and so on. Again '8', '9', '10', . . . will not go into the blank of 'the current in this wire is log (7 − ( )) amps' though the name of any negative number or of any positive number less than 7 certainly will go.

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REVIEWS

SEMANTICS

THIS volume[1] brings together thirteen essays by eleven authors, all of which have been previously published in one form or another, mainly in American journals ; and there is a brief introduction by the editor.

The word ' semantics ' has been used in several ways, particularly in recent years. M Bréal took semantics as a philological study, while Korzybski proposed it as a cure for all the ills of the world. There is, then, a certain confusion about what semantics is. As a scientific discipline, however, semantics derives from Tarski's investigations, which were first published in Polish in 1933 and in German in 1935 under the title *Der Wahrheitsbegriff in den formalisierten Sprachen.*

The present collection of essays begins with ' The Semantic Conception of Truth ' by Tarski, published in 1944. This is a short version of his previous work ; and it has already become a classic of the literature of modern logic. The achievement lies not only in having given a materially adequate and formally correct definition of truth, but equally in the method by which this result was obtained. The age-old controversy between the correspondence and the coherence theories of traditional philosophy is swept away. A conception of truth is given which is closer to ordinary usage than either theory can provide. The solution is made possible, however, only by reconstructing in logical terms the vague and ambiguous expressions of ordinary language. For such a language—though in some sense open, since it allows us to introduce new terms and to change usages —is in fact semantically closed. It contains, apart from its expressions, also the names of these expressions, as well as semantic terms like ' true ' referring to sentences in this language ; at the same time it is assumed that the laws of normal logic hold. This creates the antinomies, for example, of the Liar : natural language is logically inconsistent. Problems of truth and of meaning can be strictly solved only for formalised languages.

This result is of import not only to philosophy but also to science. And, indeed, the history of science shows how our theories become more precise with progressive formalisation, in accord with the semantic conception. Semantics arose from the study of the foundations of logic and of mathematics. While ordinary logic was always felt to be too narrow since it deals only with the formal properties of expressions, semantics considers also the referential function of language, that is, how and what our expressions designate. Semantics is then the theory of logic, in the wider sense, which is capable of explaining the methods of science.

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[1] *Semantics and the Philosophy of Language.* A collection of readings edited by Leonard Linsky, The University of Illinois Press at Urbana, 1952, pp. ix + 289, $3.75.

The subsequent articles in this volume range over a variety of topics, though they centre on the problem of meaning ; some of them have little, if anything, to do with semantics, only with the philosophy of language, in a rather general sense. For these essays are written from the traditional viewpoint of epistemology ; and yet it is exactly the merit of semantics to have substituted a scientific methodology for a so-called theory of knowledge which is independent of, and even assumed to be prior to, science. It is, I think, only fair to restrict the term ' semantics ' to work which has a recognisable relation to the semantic conception of truth ; otherwise the term loses its meaning.

It is impossible to discuss here in detail all the problems raised in this volume. I shall try to review the articles in groups according to whether they are concerned with (*a*) the meaning criterion, or (*b*) language systems and ontology, or (*c*) more technical problems such as the counterfactual conditional and the distinction analytic-synthetic.

There have been many attempts at giving a criterion of meaning that is to apply to factual (empirical, synthetic) sentences. Originally, the empiricist's demand for verifiability was intended to unmask certain metaphysical sentences which seem to be about facts but cannot be verified by any fact. For example, in physics questions about the motion of the aether, or about the simultaneity of distant events, appeared at one time to require a factual answer ; and experiments were devised to find it. But the experiments could not provide the answer, for the very good reason, as relativity theory showed, that these questions had no physical meaning. In other words, our theories allow us to formulate sentences which are syntactically correct but semantically meaningless.

The demand for complete verifiability, even if only in principle, was soon seen to be too strong. So only testability was demanded : it must be possible, at least in principle, to test our factual sentences even though we may be unable to confirm them. But this criterion also cannot properly do the job. As Hempel points out in his article, a criterion in terms of deductive relations will always be too strong, or too weak, or both, when it is to apply to expressions of a natural language. Therefore, translatability into an empiricist language is proposed as criterion. But this raises new questions : what is an empiricist language, and what is meant by ' translation ' ?

It may be possible to specify a vocabulary containing only logical terms and observation predicates. In this way, objectionable words like ' aether ' or ' absolute ' can be banished (as if they were naughty four-letter words of Anglo-Saxon origin). This still leaves us with the problem of translation. We usually say that we translate, for example, from French into English ; but this presupposes that we know already the meaning of the sentence to be translated. Obviously ' translatability ' is used here not in a normal, or even clear, sense of the term. It seems to me that the scientist *interprets* a sentence of an abstract theory like quantum mechanics in terms of a *laboratory language*. This involves using other, established, theories and models. And the language we speak in the laboratory is only psychologically, not logically, basic ; though a suitably enriched thing-language usually serves for this purpose, the sort of language in which we are able to express our experiences depends on our training and skill.

To find *the* criterion for *all* factual sentences belonging to a *natural* language (and even for isolated descriptive words, as some of the authors here wish) is a hopeless task. The papers contributed by C. I. Lewis, Goodman, Quine, Mates, and Marhenke illustrate this very vividly. In ordinary discourse, after all, we understand one another not only through logical, but by many other, means. Images, facts and situations, and the relation of an expression to these facts are assumed in these essays, on occasion, to represent the meaning of an expression. But meanings are not Platonic entities ; nor are they mysterious relations that link words to things, like invisible strings ; nor are meanings the things or events to which the expression refers ; nor is the truth-value of a sentence sufficient to give its meaning.

That no satisfactory theory of meaning is proposed here does not imply, however, that *a* criterion of meaning can never be given, and that the whole idea of a meaning criterion is mistaken. This would be quite unjustified. Historically, the criterion—in any of the many versions given—has in fact been of great service, both to science and to philosophy. The trouble is that philosophers, misguided by epistemological prejudice, have demanded more than a criterion of meaning can reasonably be expected to do.

Briefly speaking, the situation appears to me like this. A criterion of meaning is needed, not for speaking a language, but for speaking *about* a language, that is, when we argue about some obscure expression. The criterion merely explains in logical terms what we accept as a (logically or factually) meaningful sentence, both in ordinary contexts as well as in science ; for if we never knew any meaningful sentences, we could never give a criterion of meaning. But meaning, like truth, is a semantic concept. Such concepts can be formulated only for formalised languages ; and so we must first reconstruct our ordinary expressions in logical terms before we can apply them. We can state the meaning of an expression if we know its usage within a given context. If the sentence is analytic (logically true) we need only know the syntactic and semantic rules ; if it is synthetic (factual) we need also to know whether the semantic rules refer to something outside language. The rules both of designation and of truth are needed to specify the meaning of a factual sentence : and this is quite in agreement with what we understand by this term, normally. The criterion of meaning is not an *amulet* that miraculously prevents us from saying all the nonsense we are able to say in ordinary language. As a logical device it can only correct logical mistakes—and so the meaning criterion performs a more modest, though all the same important, task.

When ' meaning ' is interpreted according to one or the other epistemological theory, difficulties must arise, for example, how to find the meaning of an expression referring to something that does not exist. This raises the issue of ontology—the stock-in-trade of metaphysics—discussed in the second group of essays here.

We tend to use ordinary language as a thing-language ; nouns, for example, are usually interpreted as if they named things. And so centaurs, unicorns, and the whole Platonic host of universals are invented. Russell's theory of descriptions, reprinted in this volume from his *Introduction to Mathematical Philosophy* was intended to exorcise these ghosts. ' There is a failure of that feeling for reality which ought to be preserved even in the most abstract studies.'

This is not the place to go into a detailed analysis of the theory of descriptions ; it is discussed also by several of the authors of this volume. But it is easily seen, I think, that two different criteria of meaning are used in the theory. When an expression names, the thing named is taken as its meaning ; when it describes, the meaning depends on the truth of the expression. This conflict of criteria can be overcome when a semantic view is taken. All the semantic (and, *a fortiori*, the syntactic) rules governing the use of an expression within a formalised language are needed to specify its meaning.

Russell's ' robust sense of reality ' pushes him too far, however, when he says that ' logic . . . must no more admit a unicorn than zoology can . . .'. To distinguish between reality and phantasy is a task for the psychologist, not for the philosopher or logician who can only list the various senses of the word 'to be ', and try to give rules for their usage. There is a tendency among philosophers today to reject all abstract entities for fear of admitting thereby some philosophic myth ; but, in fact, this nominalism represents the same attitude as realism : it is merely the sign that is reversed.

The general question ' Are there electrons ? ' must be distinguished from the specific question ' Are there electrons in this piece of metal ? ' Or, in the terminology here proposed by Carnap, the *internal* problem as to whether electrons can be found in a particular place and at some given time presupposes that the term ' electron ' is accepted as meaningful, that is, that we have solved the *external* problem of choosing a language suitable for describing our experiences. Sentences out of context are meaningless ; it is only within a language system that we can say what meaning, if any, a given sentence may have ; and this is established by the rules of the system. But to choose a language system is not a matter of derivation nor is it decided, simply, by the facts ; convenience, convention, and imagination lead us to invent a suitable terminology ; and the system chosen is justified by success in practice, that is, if the factual sentences derivable within it are confirmed by experience. Epistemology has confused two types of questions—by assuming that one has to justify a whole system of concepts in the same way as one can prove or confirm a sentence of the system.

This sort of answer to the ontological problem has often been given ; but it becomes clearer in semantic terms. To say, then, as Quine does, that ' to be is to be the value of a variable ' is quite harmless, if correctly understood. Logic does not bring back the ontological problem with a vengeance as seems to be suggested. There are certain descriptive constants occurring in the system ; this does not commit us to any view about the nature of what is described ; and whether the system as a whole is concerned with fact or with the ' products of fancy ' is not a question that can be decided by logical means.

Finally, the last essays are concerned with more technical questions such as the counterfactual conditional and the distinction of analytic and synthetic sentences.

It can be seen from Goodman's very searching paper that no direct attack on the problem of counterfactuals can lead to success. Not only are there a great many different types of such conditionals ; in many instances they are not even truth-functionals. In ordinary language it is often impossible to say what meaning, if any, the conditional is to have, and for what purpose it was uttered. The subjunctive conditional ' If I had wings, I could fly ' is certainly not intended as implying a connection between the antecedent and the consequent, that is, as demonstrating the truth (or falsity) of a law—if it is one—that all winged beings can fly. But physical laws, when considered as counterfactual conditionals, for example, Newton's first law, certainly do serve as implications. Yet to say ' If no forces act on a body, it remains in a state of rest or of uniform motion ' would make the law vacuously true since the antecedent is factually false.

No physicist would want to accept Newton's law for such a reason. But this difficulty can be obviated, I think, when we consider a scientific theory as representing—at least in principle though not in fact—a semantic language system. Among the possible states of the world it describes there is also frictionless motion ; and so, within the theory, Newton's law is a proper conditional, with a true antecedent. The system of concepts, or the theory as a whole, is idealised ; and the question is only whether we can approximate the experimental conditions so that the theory is applicable.

It is not necessary to draw the pessimistic conclusion, as far as scientific laws are concerned, that in order ' to establish any counterfactual, it seems that we first have to determine the truth of another '. If laws are taken as expressions belonging to a theory, or formalised language, they can be interpreted as ordinary conditionals ; and the vicious circle is broken. But one has to agree that ' the consequent seldom follows from the antecedent by logic alone ' : in general, contrary-to-fact conditionals cannot be construed syntactically since their meaning depends on the context and on extra-linguistic information which, in ordinary language, is not clearly indicated. Once more we see that it is impossible to give an analysis of ordinary expressions without making use of a logical reconstruction of these expressions.

It has recently become the fashion to say that the distinction of analytic and synthetic (factual) sentences is not acceptable. It is an argument that has been used by the devotees of ordinary language as a big stick with which to beat the scientifically-minded philosophers.

The ancient battle-cry of a ' new revolt against dualism ' is raised here by M. G. White. But—like so many revolutionaries—he merely replaces one -ism by another, namely, Dewey's gradualism. Analytic and synthetic, however, are not ' expressive of different kinds of knowledge ' ; and they are not inherent, natural, properties of sentences. The problem is merely whether certain logical terms are adequate for classifying and analysing the expressions of ordinary language. The analogy with the temperature of bodies—hot and cold—suggested by the author is singularly misleading. It is not a question of degree, in any sense of this word. And how can one say that a sentence is partially analytic or synthetic when the whole distinction is rejected as ' untenable dualism ' !

In order to speak about an expression we need a terminology so that we can specify its usage. Our meta-linguistic schemes—either of grammar or of logic—may not always fit as well as we might wish. This problem arises whenever we *apply* any theory to whatever subject-matter ; we need think only of point-particles, or weightless strings in classical physics. But these concepts can be applied successfully in many instances. And there is no doubt that, both in science and in ordinary conversation, we put declarative sentences to two different uses : we sometimes want to refer to something outside language, and sometimes we do not.

It is, of course, true that logical concepts apply, strictly, only to formalised languages and that ordinary language is not formalised. But this does not imply that such concepts cannot be applied *at all* to natural language. Indeed, ordinary sentences are not simply either-or ; we do not always speak logically ; we cannot state clearly, on all occasions, what we mean. But if we want to give the meaning of an expression, we must speak about it and so reconstruct it in logical terms that the concept of meaning becomes applicable. And, after all, it does happen quite often that we use our ordinary expressions in a logical manner.

Logic and semantics is a theory *about* language ; it provides a methodology for helping us to understand what we say. If it is deficient in some way, we must try to improve it. Logic certainly has been improved since Aristotle ; new distinctions have been introduced. This is no reason for rejecting distinctions which in many instances do, or can be made to, fit.

Finally, there is an essay by A. Naess who tries to establish the meaning of certain concepts, for example, of synonymity, by the *questionnaire* method. This is a social and psychological study about how people use words ; it hardly touches upon the logical or philosophic issues involved.

This volume contains many valuable articles that are otherwise difficult to find. It represents, in spite of the criticism offered, that trend of philosophic thought which is closest to science. Though the selection of articles is somewhat arbitrary, there is sufficient material here to show that, just as philosophers can learn from the scientists, the scientists can also often profit from the work of the logicians and philosophers.

E. H. Hutten

SCIENCE AND RELIGION

These two books[1] (referred to below as I and II, respectively) are translations of Volumes 4 and 5 in the major work of Karl Heim, Professor of Systematic Theology at Tübingen. They therefore present what is essentially a part of a more comprehensive system of philosophical thought. They can, however, properly be considered apart, not merely because they are the only section of the work which has been translated into English, but also (and, from our point of view, chiefly) because they, and possibly a further volume not yet published, alone treat of the relations between science and religion. They can, in fact, be fully comprehended by themselves, and contain scarcely any appeal to the other volumes of the work. For German philosophy they are remarkably clearly written, and the sentences are almost entirely free from ambiguity. The only fault, in fact, which can be found with the exposition is an excessive effort to be clear ; it is sometimes rather disconcerting, when one is quite ready for the next step in the argument, to find the former one repeated two or three times. For this reason, rapid reading rather than careful pondering of each sentence is advisable in order to follow the development of thought. The translations appear to be admirable, and would be unrecognisable as such if they were not so described.

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[1] (I) *Christian Faith and Natural Science*, Karl Heim ; translated by Nevill Horton Smith. (II) *The Transformation of the Scientific World View*, Karl Heim ; translated by W. A. Whitehouse (S.C.M. Press Ltd., London, 1953. Pp. 256 and 262 respectively. 21s. each)

Professor Heim sees science not as a guide to truth but as a menace to be disarmed. This is natural, if not inevitable, since he is first of all a theologian, but it may be doubted whether it allows of the most rewarding approach to the subject. It must be added, however, that he is wholly guiltless of any attempt to distort scientific knowledge in the service of some extraneous interest. He has read widely and with understanding, and his work is therefore free from both prejudice and ignorance which unfortunately mar so many of the efforts that have been made to preserve religious creeds from destruction by science. For that reason the work demands serious consideration and appraisal from the scientific side.

In (I), Professor Heim's main purpose is to expose certain facts that lie ' on this side ' of science, facts that are therefore invulnerable to anything that science can discover or infer, and that consequently form a sort of inviolable framework within which science is necessarily confined. In the first place there is ' the non-objectivisable ego ', the ' I ' of which I am immediately aware, which stands outside anything that I can observe or conceive. Secondly, there is the relation in which this ego stands to ' the objective world ' which is the field of scientific investigation. I am at a particular position (' here ' and ' now ') in this spatio-temporal world, and although I can imagine myself elsewhere it is utterly impossible for me to be there. Thirdly, there are other such egos with whom I can communicate. ' Each of us, both you and I, must make the same demand and the same claim, namely, that we are *the* centre, the only standpoint from which everything is seen correctly ' (I, 53). This is illogical, for the word ' ego ' can be used only in the singular ; nevertheless it must be admitted. ' Logic is incomprehensibly refuted by the reality of our existence ' (I, 52). We thus have a realm of egos, and these are conceived to exist in an ' I and Thou space ', or *ego-space*, which transcends the ordinary 3-dimensional space of ordinary ideas and the 4-dimensional space-time of physical theory.

The idea of space is very prominent in Professor Heim's scheme of thought, and it is therefore important to understand clearly what he means by it. Unfortunately this is not easy. The nearest approach to a definition occurs at I, 140, where he refers to ' a space in the true sense of the word, because in it too a multiplicity of entities are arranged in order according to a definite principle '. Such a space ' may possess a structure which cannot be mathematically formulated at all '. (ibid.) If we divest the space of physics of all possibility of mathematical formulation, what remains is so shadowy that we are inclined to regard the word ' space ', as applied to the ego-space, as merely a metaphor, but this is certainly wrong ; something much more literal than that is intended. In much of the subsequent discussion it is assumed—and the assumption is absolutely necessary for the argument—that the ego-space is a non-objectivisable 5th dimension in a comprehensive 'space' of which physical space-time is a 4-dimensional sub-space. 'This transition from the objective to the non-objective space', we read (I, 146), 'is also subject to the same rule as were the transitions from the uni-dimensional space of time to plane space and from plane space to solid space.' Furthermore, just as in 2-dimensional space things are possible which are contradictory in 1-dimensional space (e.g. three points can be equidistant from a fourth without any coincidences), so when the 5th dimensional ego-space is recognised we can resolve the contradiction that I am an essential part of the objective world since I have a definite position within it and yet that I cannot be objectivised at all. I must confess that Professor Heim's ideas would have been clearer to me if he had not expressed them in spatial terms—no doubt because what seem to me the most essential characteristics of space (those which can be expressed mathematically) are here irrelevant. The whole treatment, however, is so permeated by the device of 'thinking in spaces', that I cannot attempt a re-expression without fear of misrepresentation.

We have, then, a multiplicity of egos, which are not necessarily all beings like ourselves. We can scarcely deny an ego to an animal, and by 'the principle of continuity' we may suppose that even what are called inanimate objects also have 'an inner life' and so have some extension in the ego-space. The whole creation, therefore, is distributed in the ego-space, but there no ego has a definite location. It acquires a now and a here at the point at which it makes contact with objective space-time, and the objective space-time world is the indispensable medium of communication between one ego and another. The egos may be pictured symbolically as prisoners, each in a separate cell, who cannot see one another or exchange positions but can communicate by rapping on the walls. Also, in the ego-space each ego is 'the central pivot of the world' (I, 124), and so a conflict is set up between the mutually exclusive claims of the egos, and the objective world is the arena in which this conflict manifests itself.

Both the objective space-time and the non-objectivisable ego-space are 'polar'; that is to say, a point in them is describable only in relation to another point. This is obvious enough in physical space-time; but also in the ego-space, 'I am I only by virtue of not being you or anyone else' (I, 157). Consequently, since each point or ego demands a predecessor and a successor, in these spaces 'there is no origin of the world and no perfecting of the world' (I, 157), only an endless succession and strife, without plan or possibility of cessation. Against this, however, there is the fact that some people—a minority, but not, it would seem, a very minute proportion of the whole population—are aware of a reality called God, 'the most real reality of all', which gives existence its meaning and assures them that the world has an origin and a perfecting and a plan. This experience is not an abnormality, for when the existence of God is realised the realisation takes the form of a recognition of what one has always known ; what he has become aware of is his awareness. Such an awakening is a gift of God ; nothing we can do can bring it to pass, nor, having once experienced it, can we ever relapse into our former state. All this is conceivable only if there is still another space, a ' supra-polar ' space, an additional dimension making it possible to resolve from the outside the tensions that are inevitable within the polar spaces. This supra-polar space is not God ; it is the ' presence ' of God, who is omnipresent in the whole of (presumably 6-dimensional) space. It is an error to seek God as the first or last term of the cosmic process as ordinarily understood. That process is a conflict without beginning or end ; God is the absolute who imposes on the whole of its relativity a new meaning.

Turning from these considerations, which all lie ' on this side ' of science, Professor Heim next examines the present world view of science—chiefly physics—and finds that not only does it conform to the framework delineated (as, of course, it must do if the delineation is true) but also it has come, by its own internal development, to a realisation of the necessity for that framework. ' Modern natural science ', he considers, ' has reached the frontier of its possibilities of knowledge ' (II, 169). It is of necessity limited to the 4-dimensional objective space-time, and having surveyed that field almost completely, it realises the existence of something outside. ' It belongs to our essential self to accept the hypothesis that something exists upon which we may rely unconditionally, which is absolutely secure, to which we commit ourselves not merely with the mind or with the will, but to which we cleave with our whole heart ' (II, 13). Science has sought this absolute with the mind, religion finds it with the whole heart, but the aim is the same. ' Unconditional trust is not only possible with respect to an invisible personality whom we portray to ourselves as a Father enthroned in heaven. It can also come into play with respect to a magnitude which can only be expressed abstractedly ' (II, 22). (Presumably ' abstractly ' is meant.) In the past science has relied on three absolutes—the absolute object, which it has found in the atom ; absolute position, which it has found in Newtonian space and time ; and absolute determination, which it has found in the law of causation. These absolutes have been substitutes for God ; for instance, ' faith in an absolute object or in eternal matter was a religious faith which stood opposed to faith in God and indeed ruled it out ' (II, 127). Science, however, has now realised that none of them is valid. The absolute object must be abandoned because ' a physical phenomenon . . . is never there for itself alone, as pure object. . . . Whether it is the earth itself, or an electron, which passes before me, an observing subject always belongs to the objective picture ' (II, 56–58). ' Our whole relation to reality is fundamentally altered, when the liberating recognition comes upon us that " the world is not separable from an observing subject " ' (II, 60–61). Every physical object has its ego in the ego-space, and something from that space inevitably intrudes into our attempted observation of the space-time world. Absolute space and time also have been abandoned and displaced by relativity. If two observers are in relative motion, their measures of the space and time intervals between the same events will differ, and neither set of measures has a greater claim to reality than the other. ' The realm of consciousness of a conscious being therefore posits one system of reference only. . . . If there are to be many equally justified systems there together, then there must be many conscious beings ' (II, 87). Each such being can view the world only from his position in it. ' Along with my existence, there is conferred on me an origin of co-ordinates within the space to which I belong, a world-centre, a point of perspective, from which particular measurements of space or time or motion hold good. I have not chosen this reference system for myself. It has been allotted to me ' (II, 111). The theory of relativity thus inevitably raises the question : how came I at this particular place and time rather than at another ? And this can be answered only in terms of another space. ' The decision by which I am placed within the world of experience at this precise place in unalterable relationship to it is one lying completely outside the whole objective sphere. It lies in an entirely different dimension. It belongs to non-objective space ' (II, 115). Finally, absolute determination also has been abandoned. Heisenberg's principle of uncertainty shows that the future is not, as Laplace thought, inevitably determined by the present, but that, so far as physics is concerned, it has an irremovable element of incalculability. The demand for an absolute therefore forces us beyond physics to ' the discovery of the supra-polar space, by which at one blow the last remnant of the uncertainty component disappears, in that it shows a disappearance of uncertainty about future expectation, and therefore of doubts about whether prayer is heard, and about the occurrence of miraculous healings which have been the subject of prayer ' (II, 147).

This brings us to the question of miracles. ' The whole mystery of the power of miracles is wrapped up in the word " faith " ' (II, 147-148). Faith ' does not mean any human action such as trust, or the acceptance of invisible realities as true. . . . Faith is the mode by which we exist in a space, by which we live from its resources, and are utterly rooted and grounded in it. . . . This repose in God is the sole postulate for prayer to be heard and answered and for the experience of miraculous powers ' (II, 148-149). Just as, to a 2-dimensional being, the intrusion into his surface of something from a third dimension would appear miraculous, so a man who is aware of his existence in the supra-polar space can produce miracles in the physical space-time world. The condition is that we ' believe and do not doubt that it will happen ' (II, 184). If that condition is fulfilled, miracles follow naturally. ‘ In the Bible, the miracles wrought by God never stand in opposition to laws of nature or to a causal nexus ’ (II, 189). But miraculous powers are not ‘ an experimental proof of God’s existence ’ (II, 191). A man may perform miracles in his own name only or by the aid of demons or of any inhabitant of the spaces beyond physical space-time, the sole condition being existence in those spaces. ‘ All extraordinary works of power . . . occur in conjunction with an inexplicable faith, free from doubt, that what is believed will surely happen ’ (II, 192).

Finally, Professor Heim turns to the problem of the origin and nature of life and the mechanist-vitalist controversy. Here he takes a holistic view, and advances a ‘ fundamental law ’ which is provisionally formulated as : ‘ In every fundamental structure, everything possible happens in order to build up the form appropriate to the structure in question, to maintain it and to diversify it at a higher level ’ (II, 229). This principle displaces ‘ the fundamental relationships of pressure, impulse and gravitation exerted on rigid bodies in the old mechanistic picture ’ (II, 229). It is therefore of the nature of a biological theory, and as such is not the concern of the philosopher of science so much as of the biologist. We therefore need not further consider it here.

Such is the comprehensive scheme of thought by which Professor Heim seeks to clarify the relations between science and religion and to make it possible to acknowledge the legitimate claims of both without bringing them into conflict with one another. It would give me great pleasure to be able to comment on it with a greater measure of agreement than I can, for the general line of thought is far more conformable to the scientific way of thinking than many such attempts have been. It is good to see that the scientist is not urged to acknowledge the necessity of God as a scientific hypothesis, needed to account for the creation and working of the physical universe or to fill up gaps in the scientific scheme. It is indeed difficult to understand why so many apologists for religion wish to establish their God on the present incompleteness of mechanical science, for scientists have a habit of displacing ignorance by knowledge, and then the evidence for such a God vanishes. Moreover, even so long as he survives he must inevitably be a scientific concept, definable in mechanical terms and limited by the necessity that he must account for the way in which bodies move. Equally satisfactory is the procedure by which Professor Heim attempts to identify first of all what lies ‘ on this side ’ of science before coming to the evidence that science has to offer. In effect, what he is doing is acknowledging the importance of the philosophy of science as a necessary complement of the practice of science itself. It is perhaps discouraging to the non-religious scientist to be told that the gift of awareness of the presence of God is something to which he has no access by any means at his command, but this is at least consistent with the belief that many, if not most, religious people hold, that the religious experience is beyond the possibility of scientific study. For if once it is assumed that man by any kind of searching—be it adherence to a church, by prayer, by good works, or by anything else that can be made clear to him and that lies within his power—can find out God, then the prescription can be submitted to experimental test and, if verified, made as universal and as automatic as the appearance of light when an electric switch is pressed. Furthermore, Professor Heim meets the scientist halfway by accepting the findings of science in its own sphere without question ; he does not attempt to teach the scientist his job or to limit his legitimate activities, as he sees them, in the slightest degree. These qualities combine to claim for his treatment of the subject the utmost respect from the scientific side.

It is the more disappointing, therefore, to find it unacceptable, both in its definition of the field of operations of science and of the regions outside, and in its claim that modern physics demands a recognition of that definition. It will be convenient to take the second matter first.

I find it impossible to agree with Professor Heim's assumption that the scientist's postulate—if he makes it—of something that remains constant amid all physical changes is in any sense at all equivalent to, or a substitute for, the religious man's trust in the constancy of God. If faith in eternal matter does indeed rule out faith in God, then we must conclude that—to take but one of countless examples—Clerk Maxwell had no faith in God but was at best stupid and at worst an impostor. If an absolute is posited in physics it is posited on the evidence of physical facts alone, and satisfies nothing but an intellectual need. Faith in God, in the Christian sense, I take to be something other than this. It is with much diffidence that I venture to question an assertion of Professor Heim's on a matter more theological than scientific, but it seems to me contrary to the evidence to say that faith in God ' does not mean any human action such as trust ', but is the power to intervene in the normal course of physical events in a manner impossible to the faithless man. Christian faith seems to me to be not a belief, or even a desire, that certain things shall or shall not happen, but rather a conviction that, no matter what happens, at bottom all is well. ' Though he slay me, yet will I trust in him ' ; not, ' I will trust him not to slay me ' ; still less, ' I can contrive not to be slain '. But however this may be, I can say with much more confidence that present-day physics will not bear the construction that Professor Heim puts upon it.

Take first the question of the ' absolute object '. Here the word 'object ' seems to be used in two different senses—first, as a physical particle such as an atom or molecule or electron, and second, as the correlative of subject in the subject-object relation, that is, merely as something that can be talked about and ' stays put ' while you are talking about it. Modern physical science has changed our views with respect to the object in the first sense, but not at all with respect to the object in the second sense, and it is the second sense of the word that is necessary for Professor Heim's argument. To take a common example, when we observe something through a microscope we realise now that we cannot make an unambiguous division between object observed and observing mechanism ; the microscope, for example, can belong to either according to our choice, and wherever we draw the line we must admit an interaction, which we cannot precisely evaluate, between the two. Thus the ' object observed ', if we try to isolate it from the whole physical set-up, loses its absolute character. But if we do not attempt such an isolation, but take the observation as a whole, yielding a certain result, it remains as absolutely objective in the second sense as ever. The reading of the micrometer—let us say, 23·562—has nothing whatever to do with the observing subject. Whoever he might have been he would have obtained the same result (apart from ' personal equation ', of course, which is not in question here), and that result, together with all the data recorded by all the experimenters of the past, stands objectively for physicists at all future times to ponder over but not to tamper with. It is, in fact, simply because they do accept such data as absolute and impersonal that they have been able to conclude that the object in the first sense has lost its absoluteness. If that acceptance is denied them, then the conclusion falls also. The absolute object, in the only sense of the word that will satisfy Professor Heim's requirements, remains established as firmly as ever ; if it falls, physics falls.

In the matter of absolute space and time, Professor Heim has been misled by the device which expositors so often use of speaking of ' observers ' when they mean ' co-ordinate systems '. I am inclined to think that this is the most unfortunate mischance that has ever occurred in the history of scientific popularisation, for not only Professor Heim but numerous scientists also have been victims of this error, and elaborate systems of cosmology have been built on it. Yet it is not a matter of opinion but a simple matter of fact, which every physicist must acknowledge when it is pointed out to him, that relativity has nothing to do with the particular position in space and time or the particular motion that an observer has, but only with the positions and motions he assigns to himself (that is, with the co-ordinate systems he adopts), and these he can choose as he pleases. It is incorrect to say that ' there is conferred on me an origin of co-ordinates. . . . I have not chosen this reference system for myself. It has been allotted to me.' I am on the earth, but if I am considering the dynamics of the solar system I choose an origin of co-ordinates in the Sun, as every astronomer has done for the last few centuries. If I am dealing with the stellar system I choose it at the centre of the galaxy and give myself a motion of many kilometres a second. If I am checking a sidereal clock I stay at rest and let the stars move round me, exactly as Ptolemy did. Hence I do not need to invoke a ‘ non-objective space ’ to account for my presence at this particular place, because there is no particular place. I can give my body whatever position and motion I like, and so can anyone else. It is quite wrong to say, ‘ If there are to be many equally justified systems there together, then there must be many conscious beings ’. One conscious being is enough to control all the systems. In fact there is effectively only one conscious being, because the differences in place and time and motion between terrestrial observers (and we know of no others) are utterly negligible on the cosmic scale, so that all the data with which astronomers work count as observations of one person alone. But that person, though bounded by a nutshell, can count himself king of space.

Absolute determination, as Laplace understood it, has indeed gone, because that was bound up with a particular division between the observing system and the object observed which has now become arbitrary. But determination in a wider sense—the doctrine that all our experience is capable of being represented as a rationally connected system, so that what is unknown can be inferred from what is known if the proper data are chosen—still remains the belief of scientists. It cannot be proved—it never could—but the substitution of statistical for causal laws makes no difference to it. (Incidentally, the word ‘ prediction ’, so often used in this connection, instils a false idea that time is involved, and that determination means that the future is determined by the present, and that by the past. So far as science is concerned, however, it means simply that the unknown can be inferred from the known. The calculated orbit of a newly-discovered planet, for instance, can be used to deduce its positions in the past as well as in the future, and can be corrected by comparison of the calculated places with those observed earlier but unrecognised at the time.) Whether or not everything is in principle knowable, most of it is, in fact, certainly unknown, and it is not of basic importance by what mathematical process the reduction of that quantity may be brought about. The argument that uncertainty about the future (for which we may substitute ignorance in general) demands a supra-polar space for its removal, whether or not it is valid, is thus unaffected by any modern developments in physics.

The conclusion is thus inescapable that Professor Heim's scheme of thought can claim no support from physics. But the main question is whether its fundamentals are acceptable, the considerations that lie ‘ on this side ’ of science and to which science must conform whether or not it can arrive at them by its own internal development. It seems to me that they are not. I will say nothing about the supra-polar space, since the evidence for that is admittedly available only to a minority, but let us consider the ‘ I ’, its localisation in the objective space-time world, and the realm of ‘ I and Thou ’, the ego-space. All this, says Professor Heim, precedes science, which consists solely of what can be done within the space-time world. We must at once grant the non-objectivisable ' I '—or, to make an imperfect language conform as closely as possible to ideas that strain its potentialities to the limit, let me say that I must grant the non-objectivisable ' I ', for of that I have immediate knowledge, different in character from my knowledge of anything else. To say that it is non-objectivisable is to say that I cannot talk about it, for immediately I do so it becomes the object of my speech and is no longer ' I ' but ' me '.

This brings me to the first point at which I cannot follow Professor Heim, for he succeeds through chapter after chapter in analysing what he declares to be non-objectivisable. He effectively claims both immediate and non-immediate knowledge of ' I ', where I admit only immediate knowledge of ' I ' and only non-immediate knowledge of ' me '. He does not confine this claim to ' I ' but extends it also to ' Thou '. He admits that this is illogical, but maintains that it must be granted. This is meaningless to me. Logic and experience are essentially independent. All that can be illogical are relations between the concepts chosen to represent experience, and it is our business to choose and define our concepts so that their relations are not illogical. And there is really no difficulty about this in the present case, for we can preserve the singularity of ' I ' and still recognise the multiplicity, not of ' I and Thou ' but of ' me and thee '. It seems to me that ' me ' and ' thee ' are on precisely the same footing so far as objectivisability and susceptibility to scientific examination are concerned, and their status in this respect is precisely the same as that of any object in the space-time world about whose eligibility for scientific treatment there is no question. My knowledge of all these things comes ultimately from my own experience, and my conception of them is framed to make that experience logically coherent. Take ' me ' as the extreme case. My knowledge of myself comes in various ways. The remarkable things I did at the age of two I know of only through hearing my mother talk about them. On referring to records I find that I went to school at such and such a date, left school at such and such a date, and so on. Other things I remember. My whole picture of myself is made up of reports, records and memories, and when I find myself acting in a way I would not have believed possible for me I revise my conception of myself. All this, in every detail, is true of my knowledge of you and of the house I live in. True, the proportion of memories is greater in the data for ' me ', but that is trivial since memories are in any case granted the least weight of all evidence ; they are notoriously fallible. If I ' remember ' that I met so-and-so last Wednesday and my diary tells me it was Thursday, I trust the diary.

The essential distinction between ' I ' and ' me ' is the clue to the paradox that solipsism is at the same time irrefutable and unacceptable. It is irrefutable in the sense that ' I ' am unique. It is unacceptable the moment we begin to make the deduction, ' Therefore you are less real than me ', because that conclusion does not follow. You and me have precisely the same kind and degree of reality, however we may define reality, but I have to violate grammar in order to say so, so dependent is the structure of our language on prejudices that are philosophically untenable. This essential difference between ' I ' and ' me ', which is far more profound than anything I have yet noticed emanating from the language-analysis schools of philosophy, is reflected in the different meanings of the word ' time ' as it relates to the two cases. Professor Heim brings out admirably the fact that ' I ' am always essentially at the present, and that therefore ' we must regard the entire stream of events in time from the standpoint of *now* ' (I, 59). We can then ' compare time with a long rope which is wound round a drum and which is reeled off as the drum turns ' (I, 60). ' I ' am always at the drum. What he does not make quite clear, though it is implied, is that ' me ' is distributed along a portion of the rope that has been reeled off. Perhaps even a better way of bringing out the distinction is to consider the reversal of time. The reversal of me-time would be simply a reversal of the order in which events occur in a rationally constructed extension, represented by what we see when a cinematograph film is put through the instrument in the wrong direction. The reversal of I-time would be totally different. I would ' remember ' the future, see known events coming up and happening, and then immediately become unconscious of them, knowing nothing of the past except what I could construct rationally from laws of nature derived from my knowledge of the future, by the same process as that by which solar eclipses are now foreseen.

When we fix this distinction firmly in our minds we can see how Professor Heim oscillates to and fro between ' I ' and ' me ' without apparently realising that he is doing anything invalid. In his picture of egos communicating through the cell-walls of the physical world, for instance, he is at one moment in one of the cells and at the next a spectator looking from the outside at the whole assembly. To speak in his own terms, he is at the same time objectivising I and Thou and claiming that they are non-objectivisable. The ' I and Thou ' space is in fact inadmissible, since my knowledge of ' I ' is only direct and my knowledge of ' Thou ' only indirect. There is a single space for ' me and thee and it ' ; it is a proper field for scientific investigation and is entirely objective. Science has done a good deal with ' it ', but so far comparatively little with ' me and thee '. Scientific psychology, however, is certainly in course of development, and he would be a bold man who would set limits to its possibilities. So far from science being at the frontiers of knowledge, it seems to me that it is little past its beginning. It can even now reveal unconscious motives in ' me ' of which I should have had no inkling without its achievements. ' I ' am outside its scope, but the evidence seems to show that nothing else is.

It might be misleading to end without an acknowledgment that this review scarcely touches on what Professor Heim would probably regard as the central theme of his books, namely, the supra-polar space and all that it stands for. That is true, but it is not, I think, relevant. In so far as the ideas presented are held to be significant for scientists as such—and the Preface to the first volume allows of no doubt on that point—they must be open to criticism on grounds accessible to all scientists. It is not inconsistent with this to add that there is much in the volumes that may be helpful to many whose interests are not primarily scientific.

Herbert Dingle

The Counter-Revolution of Science, F. A. Hayek (The Free Press, Glencoe, Ill.; London: George Allen & Unwin Ltd., 1952. Pp. 255. $4.00; 32s.)

In 1821 Saint-Simon published his *Système Industriel* in which he expounded the ' measures finally to terminate the revolution '. In his view, the free speculation which was required to undermine religion and feudal society was now no longer necessary ; the scientific study of society would take its place, and science brooks no argumentation. This is what Bonald meant when he referred to the products of the Saint-Simon group as the ' counter revolution of science '—the phrase borrowed by Professor Hayek as the title of his book.

The book is divided into three parts. Part I is a reprint of ' Scientism and the Study of Society ', originally published in *Economica* (1942-44) ; Part II is an account of the doctrines of Saint-Simon and his school, also published in *Economica* (1941) ; Part III is a short essay in which the similarities between the teachings of Comte and Hegel are examined—similarities noticed at the time by a pupil of Comte, Gustave d'Eichtal, who wrote to his master from Berlin in 1824 à propos of Hegel's lectures on the philosophy of history : ' There is a marvellous agreement between your results, even though the principles are different, at least in appearance '.

The most important section of the book is the first. ' Scientism ' is the mis-application to the study of society of ways of thinking appropriate to the study of physical phenomena. The whole point of the natural sciences is their ' objectivity ', that is to say, the classification of things in terms of their action upon one another, irrespective of any significance they may have for us. The whole point of the study of social action is that meaningfulness is inherent in the phenomena under investigation. Such concepts as 'money', 'the market', 'punishment', 'marriage' and so on, group together items which have the same 'meaning' irrespective of their physical natures, and further information about their physical analysis would add nothing to our knowledge of their social significance. Thus, in a sense, the two fields of study—natural science and social science—proceed in opposite directions. The natural scientist proceeds by analysis to discover the elements from which the things he is investigating are composed, while the social scientist starts with his elements immediately given in observable social interaction, and understoód by him in virtue of the fact that he himself is an inter-acting human being, and proceeds to construct theoretical schemes by means of which what he directly observes is rendered intelligible as part of a social system. The former has his 'wholes' given him and he has to find out what they are made of; the latter has his parts given him and he has to construct the 'wholes'. As Professor Hayek puts it: 'While the method of the natural sciences is in this sense analytic, the method of the social sciences is better described as compositive or synthetic. It is the so-called wholes, the groups of elements which are structurally connected, which we learn to single out from the totality of observed phenomena only as a result of our systematic fitting together of the elements with familiar properties, and which we build up or reconstruct from the known properties of the elements.'

The errors of 'scientism' spring from the lack of appreciation of this fundamental difference in procedure. According to Comte, for example, 'when man and society constitute the principle object . . . the whole of the object is here much better known and more immediately accessable'. If this were so, then an 'objective' study of society, innocent of any reference to human motivation, values, or ideas, would doubtless be possible, and we might apply the laws we could learn from such a study to the reorganisation of society according to the best principles of social engineering. This, of course, is precisely what Saint-Simon and his followers wanted to do, and, as Professor Hayek points out, it is no mere coincidence that they nearly all came from the *École Polytechnique*.

In fact, however, the social scientist as we have seen is not in this position. 'Society as a whole' does not lie before him to be manipulated, because in some important sense, there is no such 'thing'. All we 'have' are individuals interacting, and in their interaction producing unintended and undesigned results. The business of the social scientist is to synthetise such regularities as he can detect into a theoretical system. This would obviously be impossible if the interaction of many men produced no order whatever; but the question arises: supposing we can discover the order produced by the spontaneous interaction of many men, can we change it? It is here that difficulties present themselves in Professor Hayek's account. He contrasts 'two fundamentally different and irreconcilable attitudes': 'On the one hand the essential humility of individualism, which endeavours to understand as well as possible the principles by which the efforts of individual men have in fact been combined to produce our civilisation, and which from this understanding hopes to derive power to create conditions favourable to further growth ; and on the other hand, the *hubris* of collectivism which aims at conscious direction of all forces of society.' We are here in danger of confusing social theory with political policy—a danger which Professor Hayek has not wholly succeeded in avoiding. He is, as is well known, a champion of what might be called ' political individualism ', and implacably hostile on ' political collectivism '. In the first seven chapters of Part I he expounds ' theoretical individualism ' and attacks ' theoretical collectivism '. Among other things he is concerned to point out the limits of our knowledge. No one man can know all that is in fact the product of many men. No one person, or committee of persons, pretending to be social engineers, can have at their disposal the kind of information which the engineer in his proper sphere requires. From this we seem to be led to infer that no one man, or committee of men, can mould society according to their wishes. But surely, a group of men, armed with a false sociological theory, can *do* a great deal. On occasions, in fact, they have, and presumably it is for the sociologist to explain, in terms of ' theoretical individualism ' how ' political collectivism ' comes about. Professor Hayek sometimes sounds like the steward on the ship saying to the sea-sick passenger : ' You can't do that there 'ere '.

What Professor Hayek really seems to be saying is : (1) ' theoretical individualism ' is the proper method for social scientists to employ ; (2) by means of this method we can construct a social system, closely corresponding to our own, in which order is produced by the free decisions of many men interacting with one another ; (3) if we wish to preserve this order, we must not adopt the kind of policy which is implied by the false theoretical standpoint of ' theoretical collectivism ', because such a policy, from a theoretical point of view, means the end of the kind of liberty which is characteristic of our system ; and (4) since ' theoretical individualism ', which is the only proper basis of social theory, enables us to infer only short-range effects of any social action, we must, if we wish to foresee all the effects of our decisions, confine ourselves to such short-range projects, and not attempt changes on a large scale. The theoretical principles enunciated by Professor Hayek are entirely acceptable, but one cannot but feel distressed that he has not kept the theoretical and political issues apart.

The first part of the book is, as has been said, the really important part, but a word of welcome must be given to Parts II and III ; they are, as might be expected, brilliant and learned, and make a valuable contribution to the history of thought.

W. J. H. Sprott

Modern Cosmology and the Christian Idea of God, E. A. Milne (Clarendon Press, Oxford, 1952. Pp. 160. 25s.)

This is a fighting work. All the lovable Ishmael in E. A. Milne reveals itself in the spirit of these essays, which were prepared, but never delivered, as the Cadbury Lectures for 1950 in the University of Birmingham. They leave unassailed few of the tender spots on the hide of contemporary scientific orthodoxy, and our regret is the deeper that the author did not live to enjoy their reception.

The book opens with an entertaining defence of 'scientific heresy', supported with some shrewd thrusts, but designed perhaps to goad rather than to convince. Philosophers are praised for the diversity of their views, and physicists castigated (with some justice) for collective dogmatism. But, when we are told, for example, that 'a grand theory like general relativity . . . ought to be capable of standing on its own legs without the support of doubtful tiny effects' (such as the gravitational red-shift), it may be that we are not intended too seriously to ponder a reply.

It is in the second chapter (p. 23) that the title-thesis is introduced: 'If one believes that the Universe is rational, . . . there can be no residuum of irrational facts . . . save the stupendous irrationality of creation itself. To say that the Universe is rational is to say that its Creator is rational.'

Thus the hypothesis of continuous creation, for example, is at once dismissed, because no reason can be given for the choice by a rational God of any particular value for the rate of creation of matter. Laws of nature are in fact to be seen as rational deductions, the consequences of embroidering the notion of time on to the substructure of geometry.

In support of this claim Milne devotes much of the book to a welcome exposition in simple terms of his cosmological theory, taking the opportunity to make some important corrections and additions to the detailed treatment given in his *Kinematic Relativity*. The technicalities of this theory have been more competently reviewed elsewhere. Here we may note only a few of its philosophical aspects, especially as they bear on the title-theme.

In its most plausible form, Milne's theological argument might be summarised as follows:

If we try to form a mental model of a measurable universe with a minimum of arbitrary selections, admitting to our model only features for which we can show just cause, and according no special privileges to arbitrary locations within it, then we arrive at something so like our own universe that it looks as though God Himself must have followed the same line of thought in bringing the world into being.

Milne in fact regarded his results as an empirical verification of Mach's view of laws of nature as theorems implied by the material content of the universe. For him this is proof that the Universe is ' rational ', and hence that its Creator is rational.

This seems a weak and dangerous argument. If a rational God is permitted the ' irrational ' act of creation itself, then why not others ? And am I not being a little presumptuous if I define as irrational, divine choices for which *I* can find no sufficient reason ? It would seem proper to claim that the Universe as found indeed makes sense on a theistic view. But that ' the natural mind ' should understand any aspect of the mind of the Creator must surely at most rank as a somewhat surprising bonus, and is anything but a premiss of the Christian doctrine of God, which rests, as Milne would doubtless have agreed, on quite other and more convincing grounds.

On a less metaphysical plane, those of us who find Milne's concepts of t- time and τ-time somewhat mystifying may find new light in the simplified treatment that he offers here. But it is still tantalisingly difficult to discover how many of the attributes of ' commonsense time ' belong to t and how many to τ. The τ-scale, for example, is the scale of ordinary optics (p. 144), yet it locates the creation at minus infinity, which Milne will not allow to mean that the age of the Universe is infinite in the sense of common usage. What then of the astronomer who now uses quasi-optical oscillations as his standard of time ? Is there not one good sense in which the age of the universe for him is infinite ?

In his concluding chapter Milne deals hardly with the idea of the ' heat-death ' of the universe. Quite apart from the fact that his model as a whole has no final state (even its ' age ' being relative to the position of the observer), he argues (p. 149) that the entropy of the whole universe could not in principle be measured, and is therefore a meaningless concept. This is all very well. But we may be tempted to reply that our intuitive notion of a universe reaching thermodynamic equilibrium seems meaningful enough ; if the particular and rather artificial way of measuring ' distance from equilibrium ' in terms of entropy-changes in Carnot cycles is inapplicable to the universe as a whole, this is scarcely a demonstration that the statistical implications of the Second Law become meaningless when generalised to the total system. It is these implications, surely, rather than the fortunes of this rather dull quantity $\int dQ/T$, that interest us in the question at all.

The theological speculations with which the book ends may be found least satisfactory by many Christians and non-Christians alike, though perhaps for different reasons. Milne's deistic picture of God occupying Himself by ' intervening with deft touches here and there ' raises metaphysical objections to which the theistic Christian conception of God's relation to Nature (as transcendent and yet immanent) is not open.

But as so often when astronomers seek to present theological conclusions (whether positive or negative) as if they were supported by their professional investigations, the truth would seem to be that Milne's cosmology is equally compatible with the theistic doctrine, and in fact lends only permissive support to either.

This review has dealt mainly with points of criticism. It is not intended to commend these lectures for a coherent account of the relation of cosmology to the Christian idea of God. But the book stimulates even where it least carries conviction ; it invites a refreshingly unconventional and in many ways a plausible answer to the difficult question of what we think we are doing when we make a thought-model of ' the whole universe ' ; and it offers movingly sincere testimony to a faith that finds science not only complementary but contributory to its richness.

To all whose world-picture has not ossified it will be rewarding reading.

D. M. MACKAY

Aristotle's Syllogistic from the standpoint of modern formal logic, Jan Łukasiewicz (Clarendon Press, Oxford, 1951. Pp. x + 141. 15s.)

THE first three of the five chapters of this book by the celebrated Polish logician, Professor J. Łukasiewicz, are mainly historical. They deal largely with the errors of past interpreters of Aristotle's system which are revealed when it is studied from the standpoint of the modern doctrine. The author writes (p. 47) :

> Prantl and Maier are now dead, but perhaps it would not be impossible to persuade living philosophers that they should cease to write about logic or its history before having acquired a solid knowledge of what is called ' mathematical logic '.

In the remaining two chapters the system is expounded in detail with the help of Professor Łukasiewicz's ingenious bracketless symbolism. A novel feature of the treatment is the use of the notion of *rejection* (as opposed to the assertion) of a proposition. This is applied not only to false propositions, but also to propositional functions which are neither true nor false. It is the latter that were rejected by Aristotle. This notion can be avoided if quantifiers are available, but Aristotle did not use quantifiers. The last chapter is devoted to the questions : (i) whether the number of undecidable expressions in the system is finite or not, and (ii) whether it is possible to complete the system of axioms and rules so that it can be decided whether a given expression is to be accepted or rejected. An account is given of Słupecki's method of solving these problems : the first negatively and the second affirmatively by introducing a special rule of rejection.

In his concluding section (p. 131) Professor Łukasiewicz writes :

> The syllogistic of Aristotle is a system the exactness of which surpasses even the exactness of a mathematical theory, and this is its everlasting

> merit. But it is a narrow system and cannot be applied to all kinds of reasoning, for instance to mathematical arguments. . . . The logic of the Stoics, the inventors of the ancient form of the propositional calculus, was much more important than all the syllogisms of Aristotle. We realise today that the theory of deduction and the theory of quantifiers are the most fundamental branches of logic.

That logic and mathematics should have remained aloof from one another during all the centuries which separate Aristotle and the Stoics from Boole and Frege, after both had been given such good starts by the Greeks, is one of the most striking and puzzling time-lags in the history of science.

J. H. Woodger

From Atomos to Atom: The History of the Concept Atom, Andrew G. van Melsen, translated by Henry J. Koren (Duquesne University Press, Pittsburgh. Published in Britain by Basil Blackwell, Oxford, 1952. Pp. xii + 240. 32s. 6d.)

This is a well-informed, well-reasoned and stimulating work which performs even more than its title seems to promise. At its close, the author declares that in treating the history of the concept *atom* his aim has been to give the reader some idea of what it has to teach us about the possibilities of human cognition. In accordance with this aim the philosophical background of atomic and related theories of matter from ancient to modern times is given as much attention throughout as the explanation of the theories themselves. What he means by ' philosophical background ' is explained, for example, on p. 216 : ' This background will always continue to exist because natural science is born from an abstraction which apprehends only certain aspects of reality. What is missing is added by the mind of the scientist, whether consciously or unconsciously. If unconsciously, we get a philosophical *background* ; if consciously, the result will be a philosophical *theory*. Both are the philosophic perspective in the light of which scientific theories are considered.' In a chapter on ' Science of nature, philosophy, and philosophy of nature ' Dr van Melsen describes the respective provinces of these three branches of study in a way which is enlightening and fully justifies his treatment of them (with due qualifications which he does not fail to make) as three separable things. His lucid analysis may be a useful check on the not uncommon practice of declaring that either science and philosophy are the same thing, or philosophy is nothing at all.

The historical sections of the book are divided into two parts, the principle of division being that an independent physical atomic theory was not fully possible before the end of the eighteenth century. (The emphasis is on *fully*, for the author is well aware that, in his own words, 'historical reality is not the divided whole which rigorous schematic divisions tend to make it' (p. 131).) Part I deals with the predecessors of such a theory—Greek, medieval, Renaissance and seventeenth-century—and Part II brings the story down to the present day. The relevance of the Greek contribution is handled with exceptional insight, and here, as with all the subjects of Part I, corpuscular theories are properly related to the philosophical problems which they were intended to solve. Thus Democritus' atom appears rightly as an attempt to save the reality of change from Parmenides' destructive criticism by showing it to be intelligible. The distinctiveness of Empedocles could perhaps be brought out more clearly, and even in so short a survey, one misses some reference to Pythagorean monads, as also to Anaximenes' theory of condensation and rarefaction, since the relevance of this type of explanation to an atomic view of matter becomes obvious in connection with Scaliger's theory (p. 74). Most surprising of all, Epicurus gets only about three and a half lines, which do not mention the 'swerve'. In giving reasons for Democritus' statement that non-being exists (pp. 19, 31), van Melsen seems to overlook two important factors: (i) the poverty of the language at his disposal, (ii) the necessity imposed by Parmenides to avoid admitting the existence of two kinds of *onta*.

When he turns to the Middle Ages, van Melsen does valuable service in making clear the importance of the theory of 'natural minima'[1] as a forerunner of Renaissance and seventeenth-century ideas, bringing to the task an astonishingly wide reading in the byways as well as the highways of Scholastic literature. Based on a passage of Aristotle (*Phys.* i. 187 b 28-34) which seems to have had little importance for its author, this doctrine began to crystallise in the statements of the Greek commentators about *elachista*, and was taken up and developed by Averroes and the Averroists in the thirteenth century. Its difference from Democritean atomism lies in the attribution to the *minima* of qualitative differences as well as differences of size and shape, so that its value as a preparation for later atomic views is as obvious as its Aristotelian ancestry. 'The concept *minimum* was tied to the concept *form* in the Aristotelian sense, and the concept *atom* was connected with the concept of *intrinsically immutable being without qualities*' (p. 120).

Of the modern section the present reviewer is not in a position to say more than that it provides an account of the developments of atomic theory in the nineteenth and twentieth centuries which can be read with interest and profit even without a physicist's training.

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[1] There is a curious sentence on p. 59. Van Melsen read Averroes in a Latin version, but what can be the meaning of saying that 'instead of the Greek term *elachista*, he uses the Latin equivalent *minima*'? On the history of atomic theory among the Moslems there is more that could be said. cf., e.g., S. Pines, *Beiträge zur islamischen Atomenlehre*, Berlin, 1936.

The usefulness of the book to scholars would have been increased, and its attractiveness to the general reader scarcely diminished, if in addition to furnishing more references the author had given quotations from medieval philosophers in the Latin as well as in English translation. Certain erroneous forms come as a shock and would cast doubt on the author's familiarity with his subject if this did not seem to be established in other ways. Such are Heraclitus Ponticus (p. 45) and Zarabella for Zabarella (frequently and consistently). 'Homoiomerics' is perhaps a misprint. On p. 45 Asclepiades of Bithynia should be described as 'of Prusa', since two well-known men of the name both hail from Bithynian towns. Misprints are many but not serious.

The book has been translated from a Dutch original published in 1949, and fully deserves the wider circulation which this will ensure. There must be few men qualified to write with an authority equal to van Melsen's on Greek, medieval, and modern science alike. What is more, the attention to the philosophical background, and the careful evaluation of the uses and limitations of general philosophy, philosophy of natural science, and natural science itself would alone make the work outstanding.

W. K. C. Guthrie

Sir James Jeans: A Biography, E. A. Milne, with a Memoir by S. C. Roberts (Cambridge University Press, 1952. Pp. xvi + 176. 21s.)

This biography of Sir James Jeans was completed shortly before Milne's death, and has been revised by Dr G. J. Whitrow and by Mr S. C. Roberts of the Cambridge University Press, who was a close friend of Jeans during his later years. Mr Roberts contributes a Memoir, in which he conveys to the reader something of Jeans's personality with a vividness hardly achieved by Milne in his strictly professional portrait.

Few people were better fitted than Professor Milne to write of Jeans as physicist and astronomer. Jeans will be generally remembered for his great success as a populariser of science, but it is salutary to be reminded here of the mathematical brilliance of his earlier career, and Milne gives a clear account of this work in the later chapters of the biography. Jeans's mathematical career falls naturally into two periods: prior to 1914, when his interests were mainly in atomic phenomena and statistical mechanics, culminating in his *Report on Radiation and the Quantum Theory*; and from 1914 to 1929, when he published a long series of papers and two major books on stellar structure and associated cosmological problems, a field in which Milne himself was pre-eminent, and in which the two men came frequently into contact and sometimes into collision.

What of Jeans's subsequent career as a writer on popular science, and his speculations upon its philosophical importance ? Milne describes Jeans's discovery, in 1929, of a gift for popular exposition and his decision to give up original research in order to concentrate on popular writing, and concludes that probably his greatest period of scientific achievement was over, and that he was right to exploit his hardly lesser gifts of lucid exposition and striking prose style. ' No one has the right to blame him for what must have been a difficult, though deliberate, decision.' In an interesting chapter Milne describes the subject matter of popular scientific writing during the period before Jeans. At the turn of the century Helmholtz, for instance, was lecturing on energy and thermodynamics, but at that time the possible significance and popular interest of the second law of thermodynamics had not been realised. It was Jeans who first introduced to the lay public the notion of a universal heat-death. However, like most of the wide generalisations which are drawn out of the preoccupations of physics at a particular period, this one does not stand careful scrutiny, and Milne, using arguments which are now becoming familiar, exposes the weakness of extrapolating the second law from isolated systems to the whole universe.

Jeans's attempts to show how modern science throws light on philosophical problems were not always happy, and his reception in philosophical circles was generally unfavourable. Milne does not discuss the criticisms brought against him by Professor Stebbing and others, but he does offer some comments of his own on the proper relation between science and philosophy. His view is that the major speculative problems of philosophy always remain the same, but that physical theories change, and that therefore it is not the business of science to answer philosophical questions, but it is the business of philosophy to give informed logical criticism of science. This Jeans would have been well qualified to do if he had matched his wide knowledge of physics with greater powers of philosophical analysis. But he was in general too uncritical of accepted ways of interpreting scientific theories, particularly those, like the theory of relativity, in which he had not himself made original contributions. So in his attempts to discuss such problems as perception, causality, mind and matter, he never inquired at a sufficiently fundamental level just what light science might be expected to throw on these questions.

The religious views to which Jeans was led by his interpretation of modern physics were welcomed by many as showing that the scientific spirit is not necessarily synonymous with atheism, and that, on the contrary, modern science is incompatible with the mechanistic materialism of the Victorian era. But Jeans's famous argument for the ' divine mathematician ', which is recapitulated here, does not in itself carry much conviction. He had not much of that religious insight which acknowledges the mystery of nature ; in a striking phrase of Milne's : ' Jeans's

" mysterious universe " is rather a queer collection of insoluble intellectual problems than an edifice of the dim but inspiring vistas that make a Gothic cathedral.'

MARY B. HESSE

Leibnitz and the Seventeenth-Century Revolution, R. W. Meyer, translated by J. P. Stern (Bowes and Bowes, Cambridge, 1952. Pp. 227. 25s.)

DR MEYER's book will be a bit of a puzzle to readers of this *Journal*. Any sustained attempt to explain Leibniz's mathematical and philosophical ideas to the general reader must be welcomed, even when, as here, the scientific side is almost entirely neglected—the ' seventeenth-century revolution ' of the title being not the scientific revolution which gave birth to modern ideas about the nature of the physical world, but the contemporary *europäische Ordnungskrise* which gave rise to the modern Nation-state. However, the manner in which the author sets about his enterprise will create serious problems for English readers, for he uses the full battery of jargon associated with the ' history of ideas ', and accepts without question the presuppositions on which the subject too often rests. Those who cannot follow him in this may find much of his exposition unintelligible.

This is not a criticism either of Dr Meyer's style or of the translation : both are, in themselves, lucid and readable. The difficulty is an intellectual one. For Dr Meyer all that happened in Europe at any given period (around 1700, say) is to be treated as of-a-piece ; and the salient points in any field of study or activity at this time become ' symbols ', ' counterparts ', ' reflections ' or ' complements ' of those in all others. To explain what Leibniz means by, for example, the term ' monad ', one is required, accordingly, not to indicate the part this term plays in his arguments, but to point to everything in the contemporary scene which can, with a little ingenuity, be interpreted as having a monad-like character. More, it seems, is to be learnt about ' the baroque way of thinking ' from a study of ' baroque ' architecture or ' baroque ' music (or ' baroque ' diplomacy) than from its own products. The monad is in this way presented as a symbol of ' the unbounded individualism of bourgeois life ' (p. 10), of the petty German states, in particular Herrenhausen (pp. 34-5), of the European Academy Leibniz hoped to found (p. 94), of the *savant* (p. 103), of Leibniz's own ' unique personality ' (pp. 116, 120), and of Louis XIV, as the Absolute Monarch (p. 140). The *Monadology* correspondingly is to be thought of as poetry (p. 9), as ' a description of [Leibniz's] age in theoretical terms . . . an outcome of his philosophical reflection on this political process ' (p. 25), a ' metaphysics of the community ' (p. 35), a ' transcendent political primer ' (p. 125), as autobiographical (p. 158) and as personal wish-fulfilment (p. 164). We are further told, surely a little inconsistently, both that the monad is ' a precursor of the Rutherford-Bohr model of the atom ' (p. 58) and that it is ' nothing but [*sic*] a *répresentation* . . . of Leibnitz's personality in metaphysical symbols ' (p. 120) : of whose personality, one may ask, is the Rutherford-Bohr atom a symbolic representation ?

It is the notion of ' symbolising ' that is the recurrent source of puzzlement : for Dr Meyer anything, apparently, can symbolise anything else. Thus, to give a few more examples, the ellipse and the circle are symbols of European government before and after the seventeenth century (p. 25), ' the *façade* of baroque buildings is a symbol of the will at work in infinite space ' (p. 54), ' Newton's " action-at-a-distance " is a symbol of the diplomatic activity of centres of force (the courts) and fields of force (the petty states) within the European system ' (p. 59), and ' the *superbia* of individual philosophers and schools . . . a symbol of the universal conflict ' (p. 113). After this it is no longer surprising to be told that the music of Schutz was ' informed by experience of the same issue ' as gave rise to Leibniz's doctrine of Pre-established Harmony (pp. 39-40), that the architecture of Wren ' attempted creatively to solve the problem of the *labyrinthus continui* (which Leibniz had briefly encountered in his mathmatical studies) ; and in this attempt . . . the relations of the sovereign to the people, and of the individual to the universe, were permanently fixed in brick and stone ' (p. 40), that ' force, space and will are the themes alike of Haendel's music and of Leibnitz's philosophy ' (p. 54), and that ' England's accession to power ' as a result of the Glorious Revolution of 1688 ' had its immediate parallel ' in the publication—a year before—of Newton's *Principia* (p. 32). In Dr Meyer's hands, such ' parallels ', ' correspondences ' and ' symbols ' lose all power to illumine, for the terms suffer a kind of linguistic inflation that reduces their value to near zero.

This need not have happened. Treated in a sufficiently critical manner, the history of ideas can avoid debasing its own terminological currency. For instance, one can both understand and accept Dr Meyer's suggestion that Descartes's rejection of ecclesiastical authority in the field of philosophy ' corresponded to ' Louis XIV's rejection of that same authority in the political sphere (p. 38), because there is something solid to show for the alleged correspondence, namely, that weakening of the Church which made possible both these rejections. But the suggestion that the work of Newton and that of William III similarly spring from common antecedents, so that the very form of Newton's theory—like the pattern of diplomatic relations—is to be regarded as ' a product of the times ', anyone with a knowledge of the scientific issues involved must reject as nonsense. Significantly, in one of his rare excursions into science, Dr Meyer says that ' there is no doubt that Leibnitz's conception of force [*vis activa*] corresponds to the modern concept of potential energy' (p. 118, cf. 207): as is well known, Leibniz's and Descartes's dispute about *vis viva* or *activa*, which was finally resolved by the work of D'Alembert, was concerned with what we now call 'momentum' and 'kinetic energy', and emphatically not with potential energy.

A great deal of historical information is packed into the text of this book and into the voluminous notes which follow it, and from these one can learn much about Leibniz's life and times. Leibniz's multifarious activities, as would-be statesman, would-be engineer, and would-rather-not-be historian, are marked up to his credit as signs of his 'commitment'. But the relevance of these activities to his philosophical system is assumed rather than demonstrated, so that the argument fails to convince. One wonders how seriously the author means it all, especially when he is prepared (pp. 58–9) to read Leibniz's metaphysics as politics *in cipher*. Is this not a little too like Ronald Knox's masterpiece of deciphering, the proof that Queen Victoria was the real author of *In Memoriam* ?

STEPHEN TOULMIN

Philosophical Investigations, Ludwig Wittgenstein, translated by G. E. M. Anscombe (Basil Blackwell, Oxford, 1953. Pp. x + 232. 37s. 6d.)

WHEN the *Tractatus* was published in 1922, it established Wittgenstein as a leading philosopher. In spite of the aphoristic style, and the oracular character of the pronouncements, definite methods and results were offered which have proved of lasting interest, both to philosophy and to science.

Can the same be said of the present book ? It is difficult to judge since the style has become even more hectic and the utterances still more cryptic. As so often happens, the leader became the captive of his followers, and Wittgenstein reveals himself here as a Wittgensteinian. Any consecutive and connected argument is avoided. Instead, we have something closer to *free association*, though, unlike the scientific use of this method, it remains on the superficial, intellectual level resembling its current use in literature.

According to the author : 'My aim is : to teach you to pass from a piece of disguised nonsense to something that is patent nonsense' (§ 464). And, similarly : 'What is your aim in philosophy ?—To shew the fly the way out of the fly-bottle' (§ 309).

This teaching and showing the way is done by playing language-games. But, 'How should we explain to someone what a game is ? I imagine that we should describe *games* to him, and we might add : "This *and similar things* are called 'games'." And do we know any more about it ourselves ? . . .' (§ 69).

So, anything goes : pick an expression and see where it leads to. But whatever you get out of it, you can take it or leave it. It is not a matter of logical argument. Though there may be rules, and ' false ' moves, ' what we call a rule of the language-game may have very different rôles in the game ' (§ 53). Let us play the game ; but ' Our clear and simple language-games are not preparatory studies for a future regularization of language—as it were first approximations, ignoring friction and air-resistance. The language-games are rather set up as *objects of comparison* which are meant to throw light on the facts of our language . . .' (§ 130). And how, or what, do we compare ? ' A comparison : texts are sometimes hung on the wall. But not theorems of mechanics. (Our relation to these two things.) ' (p. 205^e). What happens, however, if an ardent physicist writes Newton's laws on a piece of paper and hangs it up on the wall, as a decoration ? This must raise interesting philosophic problems.

Perhaps not ; ' For philosophical problems arise when language *goes on holiday* ' (§ 38). Of course, one could say exactly the opposite, i.e. that problems arise when language is too hard worked. No matter—both are probably right, since each expression illuminates something, though we don't know what. For ' A philosophical problem has the form : " I don't know my way about " ' (§ 123).

And so it goes on. ' Why can't my right hand give my left hand money ? ' (§ 268). Or, ' If a lion could talk, we could not understand him ' (p. 223^e). By the way, why not ? I understand quite well the lion's roaring when he is hungry. Then, there is a most interesting example, e.g. ' What is our prototype of unsatisfaction ? Is it a hollow space ? . . . For example, if we lay it down that we call a hollow cylinder an " unsatisfied cylinder " and the solid cylinder that fills it " its satisfaction " ' (§ 439). One can see here that Wittgenstein's method owes something to ordinary psycho-analysis. Finally, there is the *duck-rabbit :* a worthy exhibit for the philosophical wax-museum, to be placed next to the bald King of France. Sometimes, more orthodox problems, e.g. about sensation, or about meaning, are mentioned. We find, ' Yes : meaning something is like going up to someone ' (§ 457).

But what can we learn from all this ? No one can say, since ' The philosopher's treatment of a question is like the treatment of an illness ' (§ 255). But doesn't this presuppose that asking a question is a symptom and that the philosopher is a sick man ?

Naturally, we all agree that the *Procrustean* method of forcing language into a preconceived mould is not very useful. This is no argument against all method, or against all uses of logic. We all know also that there are more things between the philosophic heaven and down-to-earth language than logical proof and experimental confirmation. ' . . . I have changed his *way of looking at things* ' (§ 144). We first have to see the problem, that is, to see that there is a problem at all (but let us not forget that philosophic problems usually do not bother the ordinary man and that, more often than not, they are produced by the philosopher for the amusement of philosophers). Still, so far we are willing to follow Wittgenstein. But this psychological orientation is only the beginning. Here we are told it is also the end, since ' One might also give the name " philosophy " to what is possible *before* all new discoveries and inventions ' (§ 126). What usually comes afterwards, that is, the reasoned argument, is this of no interest to the philosopher ?

As an antidote to system-building one might find the language-games amusing. The wilder the example, the better : instead of talking your opponent under the table, you shock him into insensibility. It is a good game. But are there no standards or limits in philosophy ?

I shall be told, of course, that I missed the point. This is a strong move in an end-game (to remain in the terminology) but, as it turns out, not fatal. ' " You are all at sea ! "—we say this when someone doubts what we recognise as clearly genuine—but we cannot prove anything ' (p. 227^{e}). By the same *argumentum ad hominem*, it must be admitted that I am right, too.

The first, and larger, part of this volume contains material already known from the various blue and brown books. The shorter, second, part was written between 1947 and 1949, according to the editors. In the German translation of the Editors' Note this is—rather confusingly—called ' Teil III '. There seems to be no reason why the book was doubled by juxtaposing the German and English texts ; their prolixity would perhaps have been less noticeable if the size of the volume, and its high price, had been halved.

Ernest H. Hutten

Psychosomatic Medicine, Franz Alexander (Allen & Unwin, Ltd., London, 1952. Pp. 300. 21s.)

Professor Franz Alexander is well known as a pioneer among psychoanalysts in the study of disorders from the psychosomatic standpoint. It is largely due to his influence and personal work that investigations based on this approach have become widespread in the United States. He is one of the very few to have made a contribution to theory, and he has given reports on a number of cases, in papers in *Psychosomatic Medicine* and *The Psychoanalytic Quarterly*. One looked forward therefore to the appearance of the present volume.

It turns out to be a very different book from what might have been expected : it is an introduction to the subject—not at all a presentation of the author's latest discoveries or theories, as might have been foreshadowed by research over the previous seventeen years. Why was the book not called *An Introduction to Psychosomatic Medicine?* We may be disappointed that Professor Alexander did not write a different book, but it is an author's right to choose the kind of book to write ; at the same time the reader is entitled not to be misled either by the title or by prefatory statements about the long collaboration that has led to the work. All this, however, should not blind us to the considerable merits of the book Professor Alexander has actually published, nor make us exaggerate its defects.

The book consists of two parts : Part I on ' general principles ', pp. 17-80 ; Part II on ' emotional factors in different diseases ', pp. 81-271.

Part I is allegedly on general principles. In fact it is a popular talk around the subject. It nowhere reaches the B.B.C. Third Programme level ; indeed, it is too diffuse and thin to be of any use to either the first year university student or the interested layman. Evidently popular exposition and advocacy are not Professor Alexander's forte. There is no reason why they should be. It would be possible to get someone else to write that part of the book. But it is not really required, and if it were omitted the book would be vastly improved ; it would hang together, and be an excellent introduction. (Part I is after all much the shorter part of the book.)

To this criticism one qualification must be made : the last chapter of Part I (though it is given a misleading title) is good, and should be retained ; and also one other page (52) tabulating aetiological factors. The reader can safely begin at Chapter VIII. The author gives a most useful description of recent results that develop further Selye's theory of the general adaptation syndrome.

Part II discusses the emotional factors in disturbances of the following kind : gastro-intestinal, respiratory, cardiovascular, skin, metabolic and endocrine joints and skeletal muscles, and sexual apparatus (this last being by Dr Therese Benedek). Professor Alexander gives convincing extracts from case reports showing a concomitance between these disorders and emotional disturbances. This is well worth while even though the relation of concomitance remains obscure. It would have been a considerable help, even though he did not wish to enter into theoretical matters, if the author had mentioned the existence of problems and pointed out where the gaps lay in the picture he was presenting. For instance, he sums up the ' Specific dynamic pattern in essential hypertension ' as follows :—

> Hostile competitive tendencies → intimidation due to retaliation and failure → increase of dependent longings → inferiority feelings → reactivation of hostile competitiveness → anxiety and resultant inhibition of aggressive hostile impulses → arterial hypertension.

Now apart from the last arrow this describes plausibly a circular process of a purely psychical kind. The last arrow, however, is of a very different kind : it brings in a jump to the physiological. There is, of course, nothing wrong about this, but the absence of all comment on the difference prevents the reader from seeing readily where a central problem lies.

Those who are interested in the mind-body relationship should read this book (i.e. from Chapter VIII) with profit, because it will provide concrete material bearing closely on the theme. It is true that no light is shed on the fundamental relationship ; but of course illumination on this cannot come from case material but can come only from a theory. It is interesting that much more light is shed on the mind-body problem by R. O. Kapp (a professor of engineering) in his stimulating and philosophically important book, *Mind, Life, and Body*—surely one of the most precise books on the subject ever written. It is to be hoped that Professor Alexander will now give us the fruits of his long experience and let us know what his ideas are ; it would be instructive also to hear about ideas that he may have tested and found unfruitful. There is much that he could teach us if he would.

J. O. Wisdom

Studies in Logic and Probability, George Boole, edited by R. Rhees (Watts and Co., London, 1952. Pp. 500. 25s.)

George Boole died in 1864, leaving a great many unpublished manuscripts which are now in the library of the Royal Society. It is clear from these manuscripts that he was planning a sequel to his *Laws of Thought* (published 1854) ; a sequel which was to have expressed the logical ideas of that book in non-mathematical language, and to have discussed the philosophy underlying formal systems of reasoning. There is enough material among the manuscripts to give a fair idea of Boole's views on these subjects, although he never seems to have been able to express them in a form which finally satisfied him. The collection of papers which is now published is not an attempt to reconstruct Boole's intended book, for most of them are reprints of papers already published in various journals during Boole's lifetime. From the Royal Society manuscripts only three papers are reproduced, one on the calculus of probabilities, which was written before the *Laws*, and two of a more general nature on the philosophy of logic and reasoning, written after his last published work on logic. Boole's first publication, the *Mathematical Analysis of Logic* (1847), is reprinted, together with some manuscript additions from his own interleaved copy in the Royal Society collection, most of which were later incorporated in the *Laws*. There are comments on, and quotations from, some more of the unpublished material in Mr Rhees's ' Note in Editing '.

About half the book consists of papers on the theory of probability published between 1851 and 1862. In these, Boole demonstrates the power of his method, based on the calculus of logic, by solving for the first time such problems as the following :

> Assuming that the stars are distributed over the heavens at random, that is, any star is as likely to be found in any one spot as in any other, the probability that a double star such as β Capricorni would not be found is equal to p, which is easily calculable. But there is such a star as β Capricorni. What then is the probability that the hypothesis of random distribution is false ?

This latter probability had generally been taken to be also equal to p, but Boole shows in a perfectly definite and formal manner, using his calculus of propositions, that the answer depends on the *a priori* probability of the hypothesis of randomness, and the probability of the occurrence of β Capricorni in the case of that hypothesis being false. He is thus able to distinguish probability values which are formal results of the assumptions adopted, from those which can only be given a definite value by dubious *a priori* reasoning. In replying to some objections put forward by Wilabraham (whose article is reprinted as an Appendix) Boole makes it clear that probability values are relative to our state of information, and not to actual connections of events unless these connections are known. Another long paper, first published in 1857, shows how to assign a probability to a hypothesis on the basis of separate testimonies whose probable veracities are known.

All these papers are written in the clear and careful style characteristic of Boole's mathematical work. It is otherwise with the more speculative papers which are published here for the first time, but perhaps it is hardly fair to judge on the basis of unfinished drafts with which he himself was far from satisfied. The two chapters devoted to these manuscripts are fairly representative of the rest of the philosophical material in the Royal Society collection, and the questions raised in them are of interest and significance, even if the discussion is sometimes tedious and inconclusive. Boole is here attempting to show that the analogy between logic and algebra in the *Laws* is not essential to the expression of his logic, and he tries to obtain the same results without the use of algebraic symbolism. The method he suggests, but does not develop, was discovered independently by Jevons, and used as the basis of the first logical machine. Several remarks scattered through these papers indicate how far in advance of his contemporaries was much of Boole's thinking on logic. For example, he defines logic as the science of all thought expressible in language, and clearly realises that this is not confined to the logic of classes. In logic, as in his mathematical work, he sought always to develop general methods, and if he did not anywhere attempt to construct a formal logic which is wider than the logic of class, this was only because he believed that such a generalisation must be made on the basis of exhaustive study of some special cases, and a formal logic of class was the obvious first step. Another question discussed, and one which he regarded as particularly important, is whether all the steps in a train of formal reasoning have to be interpretable in the same terms as the premisses and conclusion. He states his view that this is not necessary, just as $\sqrt{-1}$ may appear in a problem without invalidating a conclusion which is expressed solely in terms of real numbers.

In his 'Note in Editing' Mr Rhees implies that the manuscripts show a marked change in Boole's view of logic since the publication of the *Laws*. He does not make it very clear what he conceives this change to be, but perhaps what he has in mind is the greater preoccupation with the conditions of applicability of a formal algebra, which is seen in Boole's attempts to express his logic in ordinary language without the use of algebraic symbolism, and in his discussions of the processes of conception and judgment as providing the material upon which reason operates. Boole's interest in logic was twofold: he was concerned with its applicability as well as its formal aspect. From the first sentence of *Mathematical Analysis* onwards, he had always realised that the two aspects were distinct and to a certain degree independent: 'the validity of the processes of analysis does not depend upon the interpretation of the symbols which are employed, but solely upon the laws of their combination'; and much of his subsequent writings on the philosophy of logic are elucidations and developments of that statement. But he never lost sight of the conditions under which his formal algebra could be said to be a description of the actual processes of reasoning, and he attached as much importance to the study of these conditions as to developments of the formalism. Unlike some of his successors, he realised that symbolism does not do away with hard philosophical thinking, but on the contrary much hard and clear thinking is required before the application of any formal symbolic system can be justified.

The editor may well have felt that little was to be gained by publishing the whole of the philosophical manuscripts as they stand. Much of the writing is tedious and vague, but then so were the early attempts of the seventeenth-century writers to lay a clear and definite foundation for dynamics, and such a period is a necessary preliminary to the clarification of basic concepts in any science. A true assessment of Boole's importance must await a comprehensive study of the history of logic in the second half of the nineteenth century.

Mary B. Hesse

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(a) Books Received for Review

Inclusion of books in this list does not preclude their being reviewed in later issues

H. G. Apostle, *Aristotle's Philosophy of Mathematics*, The University of Chicago Press (Cambridge University Press), 1953, pp. 228, 45s.

Bernard Barber, *Science and the Social Order*, Allen & Unwin, London, 1953, pp. 288, 20s.

The Works of George Berkeley, Bishop of Cloyne, edd. A. A. Luce and T. E. Jessop, Vol. VI, ed. by T. E. Jessop, Thomas Nelson & Sons, Ltd., London and Edinburgh, 1953, pp. viii + 255, 30s.

Louis de Broglie, *La Physique quantique restera-t-elle indéterministe ?* Gauthier-Villars, Paris, 1953, pp. vii + 111, 1,500 fr.

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ERRATA

We wish to draw attention to an unfortunate error that appeared in 'The Interpretation of Quantum Mechanics' by Max Born in the previous Number on p. 106, l. 3, where 'Bohr' should of course read 'Bohm'. We regret making the author appear to have thrown his ally, Professor Bohr, to his opponents.—Ed.

Corrections for 'A Variant to Hilbert's Theory of the Foundations of Arithmetic' by G. Kreisel in the previous Number have been received too late to be included here. They will be given in the next Number.

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The judges were Prof. Y. Bar-Hillel, University of Jerusalem; Prof. J. Cooley, Columbia University; Prof. C. Hempel, Yale University; and Prof. E. Nagel, Columbia University.

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